BIOGRAPHICAL REMINISCENCES OF SIR SAMUEL WILKS

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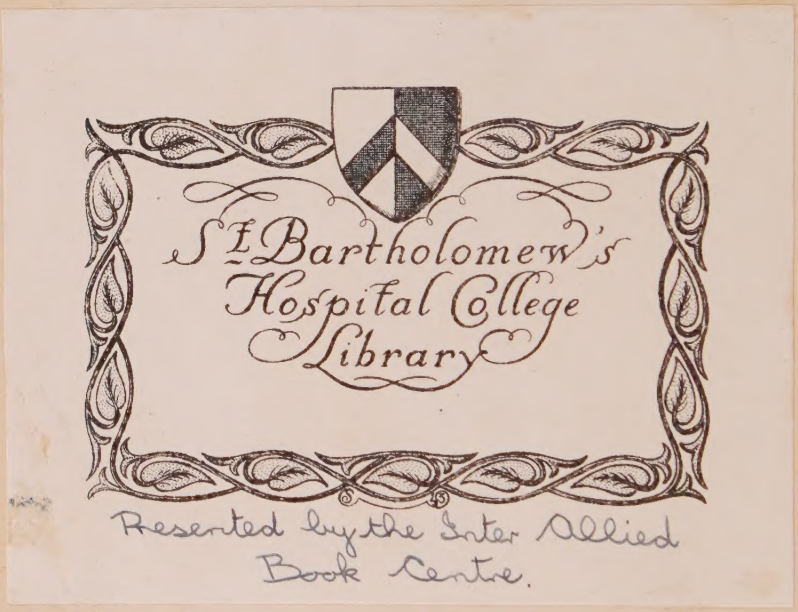

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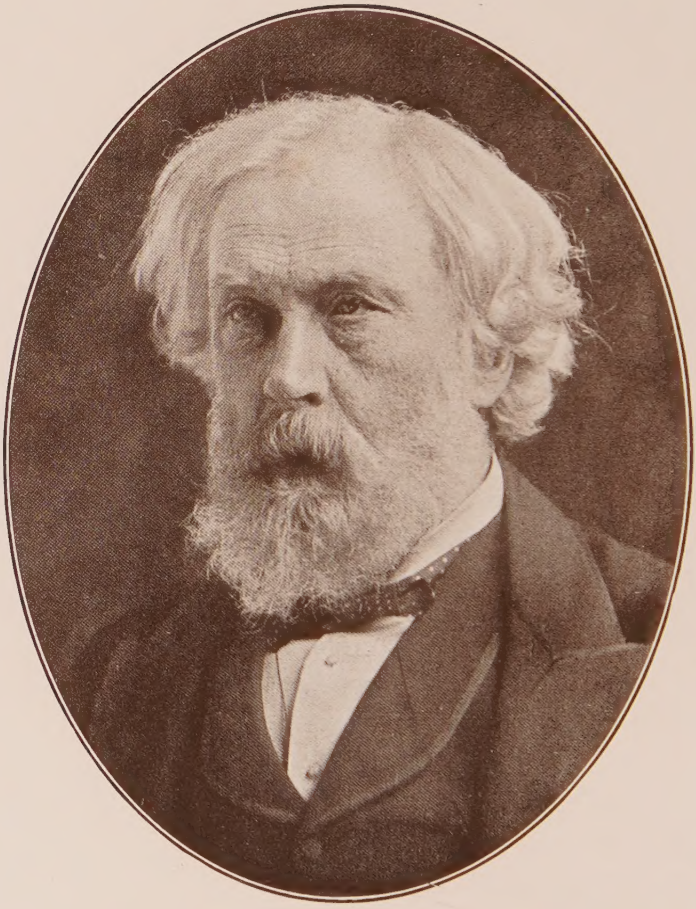

Sir Samuel Wilks, Bart., M.D., LL.D., F.R.S.

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A Memoir

by Sir Samuel Wilks,

Bart., M.D., LL.D., F.R.S.,

On the New Discoveries or New Observations made during the time he was a Teacher at Guy's Hospital.

London:

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PREFACE

THE commencement of this little book explains the meaning of the Memoir, which was *not* to give a mere personal account of myself, but rather my medical history—that is, how far I had a personal knowledge of many diseases first observed; and for this reason I went all through 'Guy's Hospital Reports' and other journals.

All this had been put away, having been written two years ago, when my friend Sir Bryan Donkin intervened and said he should like to read them. When he had done this he recommended their being immediately printed, and even published, and said that he himself would superintend and direct the work. This he assuredly did; he recommended a printer, and looked over the proofs, etc. At the present time (July 7th) the whole is printed.

Now one word for my friend, Sir Bryan Donkin; as it would have been impossible to have completed the booklet without his aid I cannot find words to express my thanks for all the advice and assistance he has given me; it could be nothing but a labour of love which was actuating him.

The portrait of myself is taken from a photograph done by Miss Williamson, sister of Dr. Williamson (the late Professor of Chemistry at University College, London) I think in the year 1884. If so, I must have been about sixty years of age.

S. W.

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BIOGRAPHICAL REMINISCENCES OF SIR SAMUEL WILKS.

I HAVE frequently been requested to write not only my biography but my personal reminiscences of all the leading professional men whom I have known. To do the latter might have had two objects: the one to note down the mere superficial appearance or some striking points in the characters of these leading men; the other to give a scientific sketch of the work in which they had taken interest and an interest in which I myself had shared. As regards the first object, I have always thought that the purely personal knowledge of a man alone cannot be of any sufficient interest to impress itself upon general readers, and this again of necessity must include a description of the outward appearance of the acquaintance or friend in question. Now, as many of such descriptions would certainly portray some oddity of manner or peculiarity of dress, they might not be very welcome to many of his surviving friends and relatives, who, indeed, might be inclined to pronounce them exaggerations or even fabrications. Having already had some experience

of this, I have determined to refrain from any attempt to deal with personalities. At the same time I quite discard the well-known dictum, *de mortuis nil nisi bonum*, which makes many biographies very pleasant reading to the friends, but to those who call themselves "outsiders" must often on perusal be very like the biographies of medical men which we read in the weekly journals, when one feels like Goldsmith's "Citizen of the World," who declared how delightful it was to read the epitaphs in the English churchyard and see how those who had departed this world excelled in every private and public virtue.

I must content myself, therefore, with writing what I may call my medical history, especially devoting myself to an account of those subjects which more particularly have engaged my attention, or, I might say, to those which I have contributed to elucidate.

It may be as well, however, to put down first the chief events of my life before I come to medical topics.

I, Samuel Wilks, was born June 2nd, 1824, at Camberwell, being the second son of Joseph Barber Wilks, cashier at the East India House, Leadenhall Street, where many of his ancestors had been before him, one of them of more especial mark, a great great grandfather who attended the trial of Warren Hastings. Within my memory I can recall the time when we did not possess in the way of domestic comfort two things which we now enjoy, and make their discoverers the greatest benefactors of the human race—the lucifer match, with the abolition

of the tinder box with its flint and steel, and the curling wick of the candle, with the abolition of the snuffers.

I first went to a dame's school next door, and then to a boys' school at Camberwell Green, and when eleven years of age, in 1835, to Rev. Dr. Spyers, at Wallop, between Andover and Salisbury, his living, nearly three miles distant, where we walked to church on Sunday.

In 1836 Dr. Spyers was appointed to the Headmastership of Aldenham Grammar School in Hertfordshire, and I followed him there. I remained three years, and the education consisted mainly of Greek, Latin, and mathematics.

In 1839 I went for a year to University College School in Gower Street.

In 1840 I was apprenticed to our family doctor in Newington, Mr. Richard Prior, with the intention of being a general practitioner. A sum of money was paid as usual, and soon I was able to make up medicines and pills for the private patients in the surgery, also to vaccinate, bleed, and draw teeth with a very primitive instrument called a "key." In the second year I was allowed to attend a course of anatomy given by Mr. Bransby Cooper at Guy's Hospital, which was only about a mile distant; and in the following year I attended the usual elementary courses, each course being separately paid for, the money being taken by Mr. Stocker, the apothecary. I afterwards had all my schedules properly filled up so as to pass the College of Surgeons and Apothecaries Hall, as these diplomas were usually taken,

although the one for Apothecaries Hall was only legally required. After a time I thought I would rather qualify for a physician and so take a medical degree. As the only one available was that of the London University I found it was necessary to matriculate, and this threw me back, although my education had done me a good turn in enabling me easily to get up the classics required, which I remember was one book of Homer's 'Iliad' and Sallust's 'Jugurthine War.'

In 1847 I passed the College of Surgeons, the same year dressing for Mr. Aston Key.

In 1848 I took the M.B., and in 1850 the M.D. In December, 1847, my master, Mr. Prior, died of pneumonia accompanying the epidemic of influenza then prevailing, so I endeavoured to keep the practice together by seeing all the patients and then to obtain as much as I could for it. Then I sold the horse, which had done me a good turn in going miles round the neighbourhood, and a man soon came who had heard of my wish, and I well remember his demeanour: he first felt the horse all down his legs, and then waved his cap in front of its eyes; after a few moments' thought, he said: "Will you take nineteen guineas for your lame and blind old 'oss?" This I readily did. I took his money, and he walked off with the horse.

In 1851 I took the membership of the College of Physicians, in 1853 I was Physician to the Surrey Dispensary, and in 1856 was elected Fellow of the College. In this year I was appointed Assistant Physician to Guy's Hospital, and in 1867 full

Physician. In 1864 I had been Physician to the Infirmary for Children in the Waterloo Road, but only kept the appointment for a year.

In 1864 I was on the Venereal Commission under the chairmanship of Mr. Skey, and held at the Admiralty. The result of this was that it was made obligatory on all public women living in garrison towns to be fortnightly examined. This measure caused such an outcry that Mr. Stanfield in 1870 proposed a Royal Commission in connection with the subject, which was held at a room in the House of Commons. This ended by the abolition of the measure, whilst Mr. Gladstone was Premier.

In the year 1870 I was made F.R.S.; in the year 1884 L.L.D.Edin. In October, 1879, appointed Physician to the Duke and Duchess of Connaught.

Commenced practice in St. Thomas's Street, and then moved to 72, Grosvenor Street, and subsequently to No. 74. From there I moved to my present abode at Hampstead when I retired in autumn of 1901.

I was President of the Royal College of Physicians for three years, from 1896–1899. Created Baronet in 1897, and in the same year Physician Extraordinary to Queen Victoria.

To resume, I shall especially dwell upon the history of the morbid anatomy of the organs and tissues of the body; and for this purpose I shall make use of the 'Guy's Hospital Reports,' as these volumes contain not only much of what I require, but were the first 'Reports' issued by any London hospital. I may here state that these 'Reports'

were instigated by the Treasurer of the Hospital, who maintained that the experience obtained at the large hospitals in London was lost to the profession generally, and therefore he hoped that an annual report would be published accompanied by the experience of the physician or surgeon, and at the same time be accompanied by an account of the post-mortem examination if a necropsy had been made.

It might be as well to say that my own inclinations tended towards the subject of pathology, or rather morbid anatomy, because it offered something positive or rather more positive than did the symptoms of disease during the patient's lifetime; indeed it seemed to me impossible to diagnose any malady whatever unless it had been previously ascertained what these morbid conditions were. Therefore a nomenclature of disease must be the foundation of medicine. I might also add that although Morgagni and other professors, both Italian and French, had existed both as teachers of healthy and morbid anatomy, yet as regards the latter the whole field lay unexplored by the modern scientific methods. This was true in my own time, for when I first examined the body of a man who had died of syphilis there was no account which I could find of any such examination which had previously been made; but this necropsy was a complete revelation to me, as it showed that the internal organs could be affected in the same way as the exterior of the body. So novel was this that some years elapsed before this great fact was accepted by the profession at large.

As my purpose in taking up my pen was to write my personal medical biography, I must necessarily say that from the very beginning of my studentship I had always had the tendency to take the most interest in all facts and cases which presented themselves in a scientific manner before me. It was for this reason that I attached myself to Dr. Barlow, who was of kindred spirit, and I remember well two cases, because I reported them so well that they were published in the 'Medical Gazette.' One was the case of a woman who had a gastric ulcer which perforated the walls of the stomach and produced an abscess in the left hypochondriac region, leaving an opening between the stomach and the cavity outside. Consequently all the signs of a hydropneumothorax existed with the characteristic splash in succussion. When the true nature of the case was discovered after death it clearly showed to me that the physical signs of chest disease depended upon certain conditions with which we were constantly meeting, and therefore we generally made a right diagnosis. But if such conditions should arise under other special circumstances we should at once be led into error, for we should naturally expect that the usual cause existed to produce them. This was a clear proof to my mind that these signs depended merely upon certain physical conditions, and we were guided only by probabilities as to the seat of the disease.

The other case was that of a man who suffered from disease and inefficiency of the aortic valves of the heart, producing a double bruit and characteristic

pulse. This case also was of great interest to me as showing how all the symptoms and physical signs were clearly owing to the imperfection of the valves and the blood regurgitating through them. It was now interesting to me to watch cases of disease where the whole of the symptoms could be so clearly explained, and more especially so as the little practice I had seen was in the hands of a general practitioner who made use of the word "congestion" to designate so many diseases, as, for example, a pain in the right side meant congestion of the liver, a pain in the chest, with some difficulty of breathing, was due to congestion of the lungs, so that this term was often used where pneumonia was the cause of the fatal malady; or pain in the head was congestion of the brain requiring the application of leeches or cold lotions.

About this time, and whilst I was still in my studentship, the subject of homœopathy was much talked about in the City of London, as a well-known financial man had written a book on the subject. My father, who was then cashier to the East India Company, asked me to read the book and give my opinion of it. This I did, and wrote out my criticism on paper. I remember the circumstance well on account of my father asking his colleague, Mr. John Stuart Mill, to read it; this he did, but declined to give any opinion on the subject, adding that the most complex and difficult subjects to investigate were those which were connected with life and the animal functions, the most difficult, he said, in the whole range of natural science. My modesty,

however, was not sufficient to prevent me offering it to the 'Medical Times,' December 7th, 1850. This was the first time my name had ever appeared in a medical paper. My arguments were not those which are so often used in connection with the infinitesimal doses of medicine, because they are so readily answered by the statement that the smell of flowers may produce a violent headache, and a sniff of ammonia may as rapidly dispel it. My argument was of another kind: first, that if the system was founded on a scientific basis it has failed to make any advance; it remained now as it was at the time of its foundation a century ago. Also that the law of healing, "*similia similibus curantur*," had been before the world for the same time, it had never been accepted by any of the well-known medical colleges or universities in Europe. This would have been perfectly impossible if it had been true, or likely to be true, for it could not be possible in any branch of science to discard for any length of time such a wonderful discovery as a new law of Nature. Again, every other school of medicine has laid it down as a necessary rule that the student should commence his work by making a foundation on anatomy and physiology, and afterwards should study these in connection with their diseased condition both in the wards and in the post-mortem room, and it is only in connection with the later study, or even afterwards, that the question of treatment can come in. But the homœopathists make the treatment or healing part first, and the great principle on which all its surroundings depend, so

it is not clear that physiology is of any value whatever.

During my studentship I was a member of the Clinical Report Society. This was a voluntary association of students, who, in the third or fourth year of their pupilage, joined together for the purpose of more thoroughly studying the cases in the wards, and so they elevated themselves above those who were "walking the hospitals," to use the expression of the time. The members of the Society met once a week to discuss the cases, and at the end of every half year a report was drawn up by the Secretary, there being one on the medical side and another on the surgical side. A room was set apart where these meetings took place, and a clerk, who was paid by the Treasurer, kept the books and did all the clerical work. I held the appointment of Medical Registrar for a year and of Surgical Registrar for half a year. In the reports which I made are one or two facts of interest, and these I shall now mention:

In the 'Medical Report' for 1853 I mention the case of a young man who had been suffering for three weeks from brain symptoms, and that after death there was found a foul abscess in the middle lobe of the brain communicating with old disease of the temporal bone in the internal ear. I give also several cases of apoplexy and paralysis. In these cases I apprehend that I was interested in the state of the pupils, for I mention two cases of apoplexy with right hemiplegia where the left pupil was

dilated and three cases of left hemiplegia where the right pupil was dilated.

Then I give an account of a woman who had epileptic fits, and during the attack she never lost her consciousness. She died after a few weeks, and on examination some tumours were found in the brain. This was of special interest to me from the fact that Dr. Bright had written in the 'Guy's Hospital Reports' (vol. i, 1836) a paper on epilepsy, in which he refers to a case in which he believed there was some disease of the brain, and therefore unlike ordinary epilepsy, where no special changes were found. He says, "My reason for supposing the epileptic attacks in this case depended rather upon a local affection than on a more general state of the cerebral circulation or excitement was due to *the degree of consciousness which was observed to be retained during the fits*." This opinion he grounded on the experience of two previous cases.

I make mention also of a number of cases of *chorea* where a systolic bruit existed, and as some of these cases had a history of rheumatic fever it was a question whether the bruit depended upon this or was immediately connected with the chorea. Whatever the answer may be, it was remarked in the 'Report' how frequently the cardiac sound and the chorea were met with together. I judge from this that the subject at that time must have been a novel one.

In my 'Surgical Report,' which was published in the year 1846, some years before, I mention one or two subjects of interest. One was that of drunken-

ness, where a man died soon after drinking a quantity of alcohol. It was impossible to obtain any accurate information as to what he had taken or the amount. He was brought into the hospital in a state of collapse and soon after died; he was perfectly insensible and his pupils were fixed and dilated, and he also had stertorous breathing. This was about ten hours after he had left home quite sober. The body showed nothing of any marked character, at least to the naked eye; all the organs appeared healthy, but the stomach still retained a considerable quantity of alcohol.

I will take the present opportunity of describing another case of alcoholic poisoning which occurred soon after this, and in this also I made a post-mortem examination. The case was that of a vagrant, aged 27 years, who, while passing along the road on the afternoon of January 28th, 1857, about four o'clock, saw a flagon containing about a gallon of brandy fall from a man's shoulder and the contents run into the gutter. Unable to resist the temptation, he stooped down and drank his fill; when he attempted to rise he staggered, and was unable to walk, so that he was already in a state of intoxication. He was taken to the police station-house, and it was understood that the stomach-pump was there applied and emetics administered. Continuing insensible, he was brought to the hospital at two o'clock on the morning of the following day, the 29th. The stomach-pump was again applied, but nothing was removed. He was collapsed, cold, and unable to swallow. He lived twelve hours after

admission and twenty-two after the debauch. His condition did not vary much, except that about an hour before his death he was roused to a state of consciousness and asked for something to drink. The pupils of the eyes were much dilated and the respiration very slow. The body was examined twenty-four hours after death. It was that of a strong, muscular young man, who had died suddenly, his previous health being good. There was no recognisable morbid appearance in any part of the body. All the organs, including the mucous membranes, were much congested, that is, full of blood, which exuded a good deal when they were cut.

To go back to the 'Report' of 1846, I mention under the chapter on "Diseases of Bone" a case of "Necrosis of the Lower Jaw," which occurred in a lucifer match-maker with suppuration and exfoliation of bone, and then I go on to say that "the disease had previously been noticed to be not uncommon in those working in phosphorus." This case, which was in the hospital in 1846, is the first, as far as I know, to be found reported in medical literature. The "phossy jaw" afterwards became so common that an Act of Parliament was passed to prohibit the use of the ordinary phosphorus, and white phosphorus was to be used in its stead in the making of matches.

The 'Medical Report' contains an analysis of the cases of fever admitted into the hospital, with reference to the distinction which Jenner had just made between typhus and typhoid, these having been pre-

viously classed together as varieties of the same disease. Both Bright and Addison, in lecturing on what was called "common continued fever," gave several varieties of it, but especially mentioned two of them—one which was complicated with disease of the bowels, and the other where this was absent. The latter, it was said, occurred in old persons, whilst intestinal disease generally occurred in the younger. Sir William Jenner, then Physician to the Fever Hospital, by a most laborious and searching investigation proved beyond doubt the absolute distinction between the two. He showed how the typhoid was recognised by the absence of the general rash of typhus, by the longer duration of the illness, and by the usual symptoms which denote a bowel complication. The typhus, on the other hand, ran a shorter course, and the whole surface of the body had a purple mottling. Jenner having just published his views, I at once commenced to examine the cases in the wards more thoroughly, and at the same time, being Physician to the Surrey Dispensary in the neighbourhood I had a good opportunity of enlarging my experience, especially as typhus fever was very prevalent in some of the worst parts of Bermondsey. I was enabled, therefore, to watch them closely, and often to obtain post-mortem examinations, although, of course, with much risk, as typhus was highly contagious. These observations proved the absolute certainty and correctness of Jenner's observations. I need scarcely say that these views were not at once accepted at Guy's Hospital, as Addison had been teaching for years

that the two maladies were only varieties of the same disease, but it was remarkable that the same scepticism was possessed by Gull; the latter, however, was Addison's pupil. It was not long before the time arrived when no further discussion was needed, for typhus began quickly to depart from the metropolis and subsequently from the whole kingdom.

It may be advantageous, before I proceed further, to give a brief account of Guy's Hospital School, which had not been established many years. Previous to this time the two schools of Guy's and St. Thomas's had been united under the name of the "Borough Hospitals," but when Mr. Benjamin Harrison became treasurer he was determined to have a school of his own, so he resigned his governorship of St. Thomas's and then commenced to rear his own medical school. He, with the sanction of the governors, lent a large sum of money for the purpose, and the teachers who were to gain the benefit were to pay the interest. After his first duties to the hospital itself by cleansing and improving the wards, he turned his attention to the school. He was only twenty-seven years of age when he became Treasurer, and held the office for nearly fifty years, and so saw the great object of his life more thoroughly and effectually fulfilled than his best hopes had ever anticipated. His father had been Treasurer before him, and having assisted and gone through a kind of apprenticeship to the work of a hospital he was thoroughly prepared for such a responsible office. He is reported to have

said that "the success of Guy's School was as dear to him as the apple of his eye."

I have already spoken of the Clinical Report Society, and have taken some important facts from them which I think are worthy of notice, but the Treasurer wanted something more, and that was that all the more important cases in the hospital should be reported for the benefit of others, for hitherto the hospital had been of no value except to the patients themselves or their medical attendant. It was important, he said, that their experience should be made use of for the whole profession, and this could only be done by publishing a yearly report. During the time Mr. Harrison, the treasurer, was building the museum for the new school, a young man was introduced to him, by Sir Astley Cooper, of the name of Towne, who took with him the model of a skeleton in wax which he had made at home. For this model he had had given him a medal by the Society of Arts; and having already made models in this material the Society encouraged him to continue the work, as it wished to bring the use of wax into England, as all the famous wax-work had hitherto been done on the Continent.

The Treasurer at once took Mr. Towne into his service, and found him rooms to work in. It was not long after this that the Treasurer asked Mr. Towne if he could model some eggs to explain how the young chickens were developed. This Mr. Towne did, and an account of it is to be found in the 'Guy's Hospital Reports,' accompanied by some very beautiful drawings.

It might be here mentioned that the Treasurer was despotic, and therefore called "King Harrison." He was placed in this position by the very nature of the endowment. Mr. Thomas Guy, having quarrelled with the Governors of St. Thomas's, of which he was an important member, having built one of the wards, was determined to build a hospital near it with his own money, and under his own direct charge and management. This he did, and no such liberal endowment had ever before been made by a private citizen of London. The trustees were made into governors by Act of Parliament, and they, with the Treasurer, ended the affairs of the institution in a very arbitrary manner. The clauses in Guy's will are still in force, but seldom used except under special conditions, one of which unfortunately occurred of late years.

I might say that previous to my time no systematic work had been done in pathological anatomy. Previous to this, a special petition had to be signed by the medical officer in attendance before such examination could be made. This was done only to verify, or otherwise, the diagnosis made during life, and which applied more to surgical cases.

A case of death from inveterate syphilis had never been examined before my time, as the cause of the disease was sufficiently evident. When, however, this was done, and it appeared that the internal organs were affected as well as the outside of the body, it was regarded as a new discovery. This will ever be remembered by me as bringing with it one of my chief honours.

The following account of a series of cases of morbid anatomy is mainly drawn from the 'Guy's Hospital Reports,' because the earlier volumes well represent the time when post-mortem examinations were not systematically made. After a description of the contents of these volumes, up to the time when I ceased to work at pathological anatomy, I mention instances of diseases which were first observed during the same period, which found no place in the 'Reports.'

Although morbid anatomy might have been called a new branch of science, it would have been impossible for the same advance to have occurred in clinical medicine if it had not been for a new invention in the form of the microscope. To show how novel this is, there is a paper in the 'Guy's Hospital Reports' by Mr. Bransby Cooper, one of my own teachers, pointing out that the microscope may be used for the benefit of medicine and especially in diagnosis.

Nothing but this instrument, which allowed us to look into a new world, could have taught us the minute structure of organs, and, later still, afforded us a knowledge of bacteria, or the visible cause of the material which produces what we had called specific complaints.

So remarkable has been the progress of scientific medicine that it has been declared that this progress has been as great during the Victorian reign as in the whole history of the world before. This is probably true, seeing that our old universities taught from Galen and Hippocrates and even Aristotle up to

quite a recent period; indeed, we know of no systematic book appearing in England before the publication of that of Sydenham. This progress in medicine no doubt was equalled by that of other sciences, but with this I am not here concerned.

I begin now to give some account of the 'Guy's Hospital Reports,' selecting those papers for notice which seem most important as bearing upon the question of novelty.

Vol. 1, 1836 (Guy's Hosp. Rep., Series 1).—In this, the first volume of the 'Guy's Hospital Reports,' there is an article by Dr. Bright on "Disease of the Arteries of the Brain," and he shows how this condition is generally associated with apoplexy and softening.

He mentions also that in many cases of other forms of disease of the brain he has often observed a granular state of the interior of the lateral ventricles, and the existence of cysts in the choroid plexus.

He then gives the case of a young woman, whose limbs were almost powerless and at the same time contracted, although they could be relaxed on using force. Death was due to pulmonary disease, and on a post-mortem examination the whole of the spinal cord was found covered with a number of cartilaginous bodies like small drops of tallow dropped in water. Dr. Bright evidently associated that condition with the symptoms, although he does not actually say so.

Then he gives a very interesting case of a man, aged 37 years, who had laryngeal and other symptoms of a syphilitic character, but came into the hospital for fits. Dr. Bright's diagnosis was the

following, and this is printed in italics: "*disorganisation affecting the cineritious substance of the brain and membranes on the left side, and probably influencing the deep seated portion of the posterior part of the corpus striatum.*" The patient died soon afterwards and there was found suppurative inflammation between the brain and membranes. Bright, remarking on the case, said he believed "the epileptic attacks depended on a local affection of the brain rather than on a general cause, as he had observed in these cases a degree of consciousness existing during the fit, and that the fits themselves point to the membranes and surface of the brain being the part most affected." When giving the symptoms Bright alludes to the speech being affected, but whether it was associated especially with the left anterior lobe is not recorded. He probably had not observed it.

In this same volume Bright has an article on "Jaundice," and divides the subject into four parts. One of them is on inflammation of the liver, and he gives a very accurate account of what was afterwards called "acute atrophy of the liver." He speaks of this disease as generally rapidly fatal, and on post-mortem examination the liver was found much diminished in size and the gall-bladder devoid of bile, or only a trace of it found in the mucus secreted from its surface. He says, "When the disease is terminated early in its course the whole liver feels rather soft and fluid, the surface appears variegated, of a light yellow colour mixed with patches of a dark red or purple. Certain portions project above the rest, which, when cut through,

often prove to be of a softer texture and even to be undergoing a process of change or disorganisation, whilst other portions of the same kind are intermixed throughout the whole substance of the liver, although occasionally the yellow portions are harder than the surrounding substance." The plate accompanying this description represents, he says, a portion from the acute margin of the liver of the patient who died under his care with this disease, without there being any mechanical obstruction to account for the jaundice. It is said also that the lighter parts in the drawing were of a bright yellow colour and slightly raised above the surface, whilst the darker parts were red and sunk below the surface.

The patient, Elizabeth —, was aged 28 years, and had had several children. She was admitted for jaundice and spitting of blood on January 4th, 1832. She had good health until a month before her admission, when she was attacked with pains in her limbs, constant sickness and occasional vomiting of dark blood. From the first she was yellow, and in a week became deeply jaundiced; the motions were dark. In spite of all means used to sustain life she succumbed on the following day, apparently from loss of blood. The body was of a bright yellow colour, with a few ecchymoses on the arms. All the organs were stained with bile, as was the secretion which flowed from the lactiferous tubes on cutting through the mammary glands.

These cases of Dr. Bright I mentioned in connection with two others in a paper I sent to one of the medical journals commenting on the fact that all

the patients were women, and had previously been delivered of children. The connection was not very clear, but it seemed as if there was some poison in the system, probably of a puerperal nature, which was acting so deleteriously on the liver. It is also remarkable how phosphorus produces a similar condition, as Fagge says in his work on 'Medicine' (1885): "The liver is found to be altered exactly in the same way as in cases of acute yellow atrophy, and it is thought that phosphorus poisoning may be one of the causes of acute yellow atrophy."

This case is very fully reported, and should be read by all who are interested in this disease, especially as I believe it is the first case giving so full a description of the disease now known as "acute atrophy of the liver."

In the same volume is a paper by Dr. Addison on "Fatty Degeneration of the Liver." He describes its appearance, and then says that it has attracted but little attention in this country, but not escaped the notice of Louis and other physicians in France, who have especially connected it with phthisis. Dr. Addison gives several cases of this fatty degeneration which he has found after death but throws little light on the causes of it.

This first volume contains a paper by Dr. Benjamin Babington on the value of re-vaccination, gained from his experience from the Asylum of the Deaf and Dumb, to which he was physician.

On his weekly visit to the asylum in February, 1828, he found a lad, aged 16 years, with an eruption, which in a day or two was seen to be undoubted

smallpox; he had a very good vaccine cicatrix on the arm. As the boy had been home for the Christmas vacation it was pretty clear that it was at that time he caught the disease. All doubts were removed as to the nature of the affection when a few days afterwards another lad, aged 15 years, had the same symptoms and rash. The cicatrix produced by vaccination was very evident. As seven cases had occurred in the neighbourhood of the asylum he determined to vaccinate all the children. Not relying on dried lymph, he got his friend Mr. Wheeler, of the smallpox hospital, to procure some fresh lymph, so that he might perform the operation direct from the child. In the meantime one of the boys just mentioned died. Mr. Wheeler took some children with cowpox to the asylum and vaccinated all the children, 230 in number. They had all been vaccinated in infancy or had had smallpox. In all of them some kind of vesicle or elevation appeared, and, in some, good maturated pustular vesicles. After this general vaccination not another case of smallpox or varioloid disorder occurred. Dr. Babington adds, after relating the above history, "I therefore recommend with confidence those who have only once been vaccinated to have the operation repeated after an interval of a few years."

In April, 1833, after an interval of five years, the establishment, with the exception of teachers, had altogether changed its inmates, when smallpox again appeared in the asylum, and all the children, amounting to 113, were re-vaccinated. Among these there were several in which the operation had

no effect; they had been before vaccinated, and had had either smallpox or been inoculated (with smallpox). A considerable number had never been vaccinated at all, and therefore were ready to take smallpox.*

This paper is one of especial interest, as it is, I believe, the first good account proving the necessity of re-vaccination. It was written more than eighty years ago.

In this volume also may be read the very first article ever written, as far as I am aware of, on the "Excision of Joints," by Mr. Blackburn, a pupil of Mr. Aston Key. The latter had just performed his first operation of this nature on the elbow-joint, thereupon Mr. Blackburn worked up the whole history of this new surgical operation, referring to many isolated cases of the kind both in this country and abroad during the previous century. If anyone wishes to become acquainted with the literature of the subject he cannot do better than read this paper. At Guy's Hospital I believe it was more particularly developed, but quite of recent years, by my colleague, Sir Henry Howse.

I might here draw attention to the fact that Shakespeare, amongst his other remarkable prophetic sayings, seems to have had in his mind this very

* I may, in illustration of these statements, mention my own case. When my household were re-vaccinated, and I must have been nearly sixty years of age, I was told that I need not be included as I had already had smallpox when an infant, evidence of which was marked when I was younger. I was, however, vaccinated, and two punctures were made on the arm. In the usual time two vesicles appeared, terminating in the ordinary cicatrix.

operation which I have spoken of as a novelty. In "Richard II," the Duke of York, in speaking of improving the condition of the State, says :

> "This fester'd joint cut off, the rest rest sound;
> This let alone will all the rest confound."

In this volume is contained an account of Sir Astley Cooper's experiments on rabbits and dogs in reference to the anastomosis which takes place when the large blood-vessels are ligatured. By means of these experiments he saw the feasibility of tying some of the largest arteries in the body, and he gives an account where this was done in the subclavian, carotid, and iliac arteries by himself and also by Mr. Aston Key at Guy's, as well as by other surgeons. These cases were also related in a paper he read at the Royal Medical and Chirurgical Society, and printed in their 'Transactions' for the year 1813.

Sir Astley Cooper also relates how on January 28th, 1831, he tied both right and left vertebral as well as the right and left carotid arteries in a dog. The animal appeared insensible, or rather as if intoxicated; it had difficulty of breathing, pupils dilated and volition diminished; it then fell down and had spasmodic twitching. In an hour and a half it staggered across the room. On the 29th it was still dull and indisposed to move. On the 31st it walked round the room and ate a little. On February 1st it was much better, and from that time it gradually recovered and afterwards became a good house-dog. After its death the blood-vessels

were injected and the parts dissected, which showed the collateral circulation well established.

In another experiment Sir Astley Cooper ligatured the carotid artery on both sides. Very little disturbance was manifested except quickening of the respiration for a short time, for the dog appeared as well as usual on the next day.

Then in another experiment he ligatured both vertebral arteries. When he had tied the first there was some difficulty of breathing; when he had tied the second the difficulty was increased; respiration was at first slow and afterwards quickened. In two hours the breathing was laborious and the heart beat quickly. On the following day it was much the same, the heart being quick. On the next day the breathing was slow and heaving. So it continued until the sixth day when breathing became much slower and the heart beat rapidly. On the seventh day the animal was found dead.

Then he reversed the experiment and showed that although obstruction of the carotids had a certain effect upon cerebral operations, yet that a compression of the vertebrals afterwards put an entire stop to the functions of the brain.

Some very interesting results were shown by ligaturing temporarily the vertebrals and then allowing the blood to circulate; when tightened the same severe symptoms were produced as just now narrated. When the ligature was removed the animal at once recovered. This was done several times and always with the same effect.

Then he experimented on the nerves. He placed

ligatures on the grand sympathetic nerves on both sides; the respirations became very quick and irregular and the heart's action became very quick. The animals either slowly died or quite recovered. Then in another rabbit he divided the phrenic nerves one after the other in immediate succession. The breathing became instantly excessively laborious and in twenty minutes the rabbit was dead.

In the same volume Mr. Wilkinson King gives his observations on the thyroid gland. He enters into the general history of the subject in reference to its nature in men and other animals. He speaks of the arrangement of the lobules and the nature of the large cells and its general structure; also the transmission of its peculiar fluid to the lymphatic and venous system. The knowledge of these particulars, he says, depended solely upon his own investigations and experiments. He enters into particulars about the shape of its lobules in different animals, of the various diseases found in man, and says also that the morbid anatomists should by no means neglect the inspection of the thyroid gland, which may undergo many morbid changes. Amongst these is atrophy. "In this case the peculiarities of the individual were fully sufficient to indicate the importance of the defective part." Unfortunately Mr. King does not say what these peculiarities were. He speaks of the fluid from the gland; although resembling the serum of the blood it had a peculiar look of its own; he considered it to be a variety of albumen; it was completely coagulated by boiling. He then speaks of the large arterial supply to

the thyroid, which was very remarkable, but in spite of this was quite pale in colour. The veins also were very large and numerous. These are found diffused through the lobules, and the larger ones possess valves.

Sir Astley Cooper also has some remarks to make on the same subject. He had given considerable attention to the anatomy and physiology of the thyroid body, and had remarked on the large supply of blood which it possessed as well as lymphatics. He also noticed the large amount of fluid which ran out of the gland when it was cut, and this, being coagulable by heat, he regarded as albuminous.

Mr. Wilkinson King's writing is somewhat involved, but it is evident that he thought the thyroid produced a fluid which was essential to the integrity of the blood, although there is no allusion to any defect in mental power or the other symptom known under that erroneous name, "myxœdema."

The first volume also contains a paper by Dr. Hughes, read before the Physical Society in 1835, on "Pericarditis." He says: "I cannot forbear mentioning it as an extraordinary circumstance that a disease so common as rheumatic pericarditis, which we now witness almost daily, should have been known to the profession of this country only for a period short of thirty years, and that even the French should at the present time, or at least previous to the publication of Bouilland's very recent work, have, it appears to me, been very generally ignorant of the fact that rheumatism is generally the usual, or at least the most frequent

cause of the complaint." "It is another proof," he continues, "of the rarity of original observation and our proneness to tread in the steps and be influenced by the opinion of those who have preceded us." Then follows an account of the symptoms and physical signs accompanying pericarditis by Dr. Hughes. There seems to have been at that time considerable difficulty in diagnosis, the signs being associated with other morbid sounds. Dr. Hughes confesses there is sometimes such a difficulty, although no one can deny that the "*frottement*," described by Laennec, is the true and most characteristic sound usually produced by the opposed roughened pericardial surfaces. He gives a very interesting extract from the writings of Dr. Latham, in which the latter says: "Here it is remarkable, first, that a vital organ should sustain an acute inflammation without any symptom whatever immediately referable to itself, and that organ the heart; and secondly, that it should impart expressly to another organ, not the mere symptoms of common derangement, but the authentic symptoms of inflammation, and that organ the brain."

These paragraphs which I have transcribed show what our knowledge was at the time this essay was written; in fact it is the first account of it as far as I know.

Before proceeding further with my survey of the 'Guy's Hospital Reports' I will make some extracts from the preface of the first volume written by the editor:

"The great, and, indeed, the only source of our

knowledge of Nature is experience. By this we mean, in the words of an eminent philosopher of the present day, 'not the experience of one man only, or of one generation, but the accumulated experience of all mankind in all ages, registered in books, or recorded by tradition.'* Now in some departments of science the phenomena are within our control, and we can produce them at pleasure. In others, on the contrary, they are beyond our influence, and our only opportunities of witnessing them occur when they present themselves to our notice. In the former we can obtain our experience as we need it, by the aid of experiment, and in these the progress of knowledge has been sure and steady. In the latter we are compelled to act the part of powerless spectators, and our highest wisdom must consist in treasuring up the experience that is doled out to us by the course of events; in these it has been uncertain and irregular. What advances, for instance, have been made in chemistry since mankind have agreed in regarding it, as it really is, a science purely experimental? While of earthquakes and volcanoes and many of the great phenomena of nature, we know little more than did the ancients.

"Now medicine, by which we mean the investigation of the phenomena of disease and the influence of remedies upon it, is nearly, if not entirely, a science of observation. In our inquiries concerning the history of disease experiment can scarcely at all enter, and with regard to the action of remedies, although every exhibition of remedial agents is in

* Sir J. W. Herschel.

some measure an experiment, still, the caution we are bound to observe places it in a very different light from those which we perform upon brute matter for the purpose of 'asking questions of Nature.' The most important means, then, by which we can at present hope to render the progress of medicine certain and uninterrupted is the accumulation of recorded observation; that by thus collecting the fragments of the history we desire, in disjointed and, at first sight, perhaps unmeaning sentences, we may at length be enabled to arrange them in a systematic narrative. True it is that the opportunities of observing are numerous, but when the intricacy of the subject is considered, are they redundant? True it is that detached and broken sentences of the tale we wish to learn are from time to time uttered in our ears, but if we let them pass unheeded can we expect that they will be repeated at our pleasure?

"Again, the uncertainty of medicine seems to resolve itself into a want of that uniformity of phenomena which is so remarkable in the physical sciences. The want of uniformity is, we believe, merely on the surface, for no reflecting mind can for a moment doubt that the chain of causes and effects is as unbroken in the animal frame as in the unorganised kingdom of Nature, or for a moment suppose that chance interferes in vital actions any more than it can disturb the planetary orbits or control the traject of comets. The apparent uncertainty is nothing more than the necessary result of that complexity which must ensue when several causes interact one upon the other; it

arises, in short, not from any irregularity in the operations of Nature, but from our own ignorance and limited observation. If we could imagine an intelligence which could comprehend at once the physiological and pathological condition not only of every organ of the body but of each of its ever-changing particles, as well as of the fluids which circulate in its vessels—if we could, moreover, estimate in their due proportion the various forces, mechanical and chemical as well as vital, by which each of these particles may be at any given time actuated—we might be enabled to calculate its past and foresee its future condition, and to predict the result of any new agent which might be introduced into the system. But can we reasonably indulge the expectation that such an insight into the secrets of Nature will ever be attained by human intellect? We think that the present state of physiological and pathological science is far from warranting such an anticipation. If, then, it be conceded—as we think it must—that we are unable by any *à priori* reasoning to predict the course of a disease or pronounce with certainty upon the action of any remedy, does it necessarily follow that we must, as in despair, cease from all attempts to diminish that uncertainty which has so long been the reproach of our art, and to reduce it more nearly to the condition of an actual science? Every analogy which might be drawn from the physical sciences tends to urge us to a contrary course of action, and we might institute a comparison with many of them."

The editor then illustrates his arguments by

taking many instances both in physical and chemical science and contrasts the true method of inquiry with that which is purely speculative. When he speaks of experience he says he does not use the term in the popular way. When a medical man is spoken of as a man of experience he means the man who, in making observations, can eliminate adventitious circumstances and place in a clear light the phenomena themselves and the elements which are essential to them. The editor then concludes his introduction to the 'Hospital Reports' by saying:

"We offer them to the profession at large as a collection of facts drawn from the largest repository of disease in this metropolis, and as a contribution to that general stock of experience of which we have already spoken as essential to the regular and ready improvement of our art; being convinced that it is by such steps, and by such alone, that we can hope to reach an eminence whence we may look forward to the promised land of a rational medicine, although the occupation and free possession may be reserved for generations yet to come." He continues by saying that he has been speaking of every man gaining experience, but this alone is not sufficient; he should join his experience with that of others, for he may be apt to form conclusions from what he has only seen for himself; and therefore should join his observations with those of others, and thus produce a common stock of knowledge. He further says that he does not denounce personal experience, because a man's best knowledge is that which he has obtained for himself; it enables him to

appreciate a great variety of conditions which it would be impossible to communicate to anyone else.

Then I have notes taken from the third volume of the 'Reports,' in which he says in spite of the value of the reports of the clinical teaching, "the teachers are much tied by the requirements of the examining bodies, and the result is that a young man not infrequently loses the opportunity of attending the wards of our hospital at the very time when such attendance could be turned to the greatest account. He does not wish to underrate other departments of knowledge, but they should not engross so large a part of the student's time."

He then goes on to speak of the time allotted to chemistry, botany, and materia medica.

I then make another very important extract, stating that the clinical reports of the Hospital would be acceptable to others by increasing their acquaintance with one of the leading schools of the metropolis, and that they would be generally received as a laudable and promising endeavour to throw open a large emporium of knowledge, hitherto almost entirely closed. Our expectations have been fulfilled in the efficient support of our contributors, in the effect produced upon the medical school, and the estimation in which the attempt has been held by a considerable number of the professors.

I will not proceed at once to select the most important papers from the volume, but allude to the preface of the second volume, in which the editors speak of the success of the work which they had undertaken :

"Before commencing the second volume of the 'Guy's Hospital Reports' we are desirous of engaging the attention of our readers by a retrospective glance at the first, for the purpose of showing that our promises have been faithfully performed; that no false estimate was formed of our resources; that the work has been accomplished in full accordance with the spirit in which it was undertaken; and that no insurmountable obstacles have occurred to hinder our progress or disappoint our expectations.

"Amongst the various reasons that presented themselves in favour of this undertaking the following appeared to have the greatest weight. It was considered that an hospital and school of medicine so complete and extensive as Guy's, embracing both in theory and practice the whole range of sciences connected with the healing art, afforded a store of information worthy of being extended beyond the narrow limits of oral instruction; that the medical officers and teachers would gladly avail themselves of a proper and convenient medium through which to publish such facts or observations, which would with less propriety find a place in a systematic or separate treatise."

Now I shall quote from the introductory lecture given by Dr. Bright at the commencement of his course, as quite in accordance with these views of the editors, being, as he was, one of their contemporaries in the 'Practice of Medicine,' on the knowledge which can be gathered only by personal observation, and which he never could obtain from

written descriptions, such as "the pallid face of hæmorrhage—the waxen hue of amenorrhœa—the vacant lassitude of fever—the purple cheek of pneumonia—the bright flush of phthisis—the contracted features and corrugated brow of tetanus—all of which shades of countenance, with many more that might be enumerated, are distinctly recognised by the experienced eye."

Vol. 2, 1837.—In this, the second volume, is a paper by Mr. Wilkinson King, "On the Safety-valve Function of the Valves in the Right Ventricle of the Heart." The author had already read a paper on the subject before the Royal Society in 1835 showing how this always and naturally occurs in some of the lower animals. He showed also how this must always take place in the human subject when engorgement of the right side of the heart occurred, accompanied by some backward flow in the veins, as seen occasionally by their pulsation. In order to show this easily on a patient he would take a thread of sealing-wax or a bristle from a broom and stick it on a vein by means of a little tallow in a slanting direction. The slightest amount of pulsation was then made evident at the end of the lever.

A further paper on the subject is to be found in Vol. 6 of these Reports.*

This second volume contains a very interesting

* In my 'Biographical History of Guy's Hospital' I describe Mr Wilkinson King as a man of great promise, a most original thinker, and indefatigable worker, so that had he lived (dying early of con-

article by Mr. Aston Key on lithotrity. He invited the celebrated surgeon, Heurteloup, of Paris, to give a lecture and demonstration to his class at Guy's. Heurteloup had made many forms of instrument for crushing a stone in the bladder, one for hammering and another for boring the stone; these he had now replaced by an instrument for crushing the calculus. It consisted of two longitudinal valves, one fitting into the other by means of ridges or small spikes; these were drawn asunder or closed by a screw at the top of the instrument. One of the blades was fitted with a netted bag to catch the fragments of the stone when crushed. The plate which accompanies the paper gives several illustrations of the various instruments which Heurteloup had invented. It is evident from Mr. Key's criticism that he was not altogether pleased with the operation of crushing, for he speaks of the irritation, and even ulceration, of the bladder which often followed it, even when performed by such distinguished surgeons as Velpeau and Civiale.

In this volume Dr. Bright has an elaborate paper on a form of tumour in the abdomen due to "acephalocyst hydatids." He begins by saying: "That although these are regarded as independent animals, it is altogether unknown whence their ova are derived or how they are introduced into the

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sumption) he would have taken a very prominent place in scientific medicine. Guy's was most unfortunate in its earlier days of pathological work, for it not only lost its earliest promoter, Hodgkin, but his successor, Wilkinson King, who would most worthily have taken his place.

body, especially as they are confined to no particular organ. They often form enormous elastic tumours in the abdomen, encroaching on all the organs or proceeding from the liver itself." The first case which he describes is that of a woman admitted into the hospital in August, 1827, where she eventually died. He says that he "found the enlargement of the abdomen due to two very large hydatid cysts, the one originating in the liver, the other lying at its side. On opening this tumour it was found to contain nearly a gallon of a perfectly limpid fluid and had within it a number of smaller hydatids the size of hen's eggs. In places its walls were as thick as washleather. The other cyst also had smaller ones inside and some broken shreds of others. These included cysts varying in size from that of a hen's egg to that which was only just visible to the eye. The inside of the larger cysts was covered with a number of little rounded bodies of an opaque white colour, which, when separated from the surface, floated for a little time in the fluid and then slowly sank to the bottom. The coats of the hydatid admitted of an almost indefinite division into smooth and even lamellæ." Dr. Bright then goes on to say that "he borrowed a microscope from his friend Dr. Roget, who had lately obtained it from Amici (in Paris), and asked his friend Mr. Say to delineate some of the appearances the hydatid presented, using Amici's third power.

"An hydatid cyst about the size of a pigeon's egg having been carefully opened was found to contain innumerable small hydatids scarcely visible to the

eye, a little group of them looking like a few grains of sand. When examined by the microscope they were found to consist of simple hydatids which had generated others within themselves and so might be considered pregnant or prolific. The minutest hydatids were inclined to an oval form; they were all studded with innumerable points or inequalities giving them a spotted appearance, the specks being light or dark according to the duration of the light which fell upon them. These simple hydatids seldom touched or compressed one another. The pregnant minute hydatids contained from two to twenty or more within them. The larger hydatid appeared covered internally with innumerable small projections. In some cases a group of small vesicles were seen attached to the interior like clusters of grapes closely set upon a stalk. In another one the surface had a botryoidal appearance from the elevation of a number of oval bodies like compressed hydatids or they might have been little clusters."

There are two plates accompanying this paper, one representing the large abdominal cyst containing a number of other cysts, floating hydatids of various size, within them; the other plate gives the microscopic appearances of the small hydatids; hanging on the interior surface are the echinococci.

These plates display very accurately as much as the microscope could at that time show. It is evident that Bright was giving a very clear description of the disease and the microscopic appearance. This had never before been done. It was in 1827.

There is another article by Dr. Bright on "Tumours and other Diseases of the Brain." Amongst these there are two of aphasia. Such cases had been previously noticed, but it speaks well for Bright's powers of observation that he should not let such cases pass by without his recording them. One of them is a young woman, aged 30 years, in the Hospital in 1836, who had hemiplegia of the right side after an apoplectic attack. He states that she had "a difficulty of connecting words with ideas." For instance, when the doctor asked the name of her hand, she said "pin," but signified by her gestures that she knew it was not the right word.

In another case, which he saw in private practice in June, 1832, the patient was a middle-aged man, and Bright was called in to see him on account of a singular incoherence in his speech. Then Dr. Bright describes particulars as to the form which this took. "I showed him my card, and he read it with a kind of triumphant air. 'Bright; yes, you see I know it. Now, it is so odd,' said he, laughing, 'I cannot say what I want! I know all your names so well.' I then said it slowly, and he repeated it distinctly, laughing with great good humour at his own stupidity."

Vol. 3, 1838.—The first article in this volume is by Sir Astley Cooper on "Spermatocele or Varicocele of the Spermatic Cord." He begins by describing the complaint as formed by hanging tortuous veins in the spermatic cord, and that it occurs more fre-

quently on the left side, but it is of no great importance except from the effect it so often produces —of an extreme anguish of mind as well as the body, the latter being a sinking feeling at the pit of the stomach and a confirmed belief of impotence which renders life a misery. The patient is generally always taking medicine and not infrequently applies to a surgeon to have his testicles removed. Sir Astley relates a case where a surgeon removed a testis on account of the man's importunity, but he considered himself justified in performing the operation as the patient had received a blow on the testis which might have caused the constant neuralgia.

Sir Astley Cooper then proceeds by saying that the operation of tying the veins which had been proposed was too hazardous to venture upon, but suggested another plan of removing the scrotum, and then the cicatriculum would contract the skin and shrivel up the veins.

I might here remark that surgeons at that time avoided by all possible means the opening or injury of a vein as it so often carried with it some infectious matter and so produced the dreaded pyæmia. I might mention here that I once witnessed an all but fatal result from the operation of tying the spermatic veins, but this was by an embolus passing into the heart; after a sudden collapse and failure of the pulse, there was a rally in the course of some hours and then a slow recovery; the patient was a young and healthy officer in the Army.

In the next volume of the 'Hospital Reports' is a case operated on by Mr. Bransby Cooper after his

uncle's method. He speaks of the necessity of an assistant holding back the testicles close to the external ring.

This volume contains two papers by William Augustus Guy (afterwards at King's College) on the "Alterations of the Pulse by Change of Position." It is the report of various experiments.

Then Dr. Wilkinson King contributes a paper on "White Patches on the Surface of the Heart due to Attrition." He shows how these are mostly on the right and anterior surface of the heart, and seen also on the projecting coronary vessels.

A paper by Mr. Cock on "Congenital Deafness." He was Honorary Surgeon to the Deaf and Dumb Asylum, and therefore had the opportunity of examining the ears of deaf children who had died in the institution. He had already sent a communication to the Royal Medical and Chirurgical Society, and this is to be found in the nineteenth volume of the 'Transactions.' In most cases he believed the deafness was due to disease at an early period of life, for he had found all the cavities filled with scrofulous matter; in some he had found growths, and rarely malformations; these were, for the most part, met with in the semi-circular canals, and of which he gives drawings.

Mr. Wilkinson King contributes a paper "On Morbid Flattening or Compression of the Left Bronchus, produced by Dilatation of the Left Auricle."

The same writer also gave a paper on the "Causes of Strangulated Hernia." He shows a long table of

cases, giving the time when the hernia appeared, and the time also when strangulation took place. This occurred generally so long after its existence, and when the patient was getting on in years, that the only conclusion he could come to was that the strangulation was not owing to any local cause, but was due to some weakened condition of the system.

Next follows a paper by Mr. Gorham, an old pupil at Guy's, and who became, when established at Tonbridge, known as one of the most highly scientific men in the profession, having invented a new form of ophthalmoscope and a top painted with different colours, showing how these blended when the top was spun. The paper in the 'Reports' was entitled "Intussusception in Infants," and written in the year 1838. This gave a very clear and accurate account of some cases which had been under his care, and in one fatal case the description is accompanied by a drawing by Canton, showing the inverted ileum inside the cæcum. He described also the characteristic symptoms, such as the tumour often felt in the abdomen, the passing of blood, and sometimes the sloughing away of the strangulated part. He then gives the best method of treatment, which is by throwing up into the bowel warm fluids, which presses back the involved part. He mentions a case where this method had failed, and on the fifth day of the symptoms bellows were used as a last resource. Soon after violent contractions took place, and in five hours there was a copious evacuation, and recovery quickly followed.

Dr. Bright has in this volume a second paper on

abdominal tumours, which is devoted to *ovarian forms*. Then there is a third paper given to *splenic tumours*. In this the author shows so much acumen and accuracy that I shall quote a few of his sentences. He begins by speaking of the different sizes in which the spleen may be found, sometimes shrunken to the smallest possible size, and then so large as to reach the lower part of the abdomen. Then there may be found extreme softening, and in other cases the organ is excessively hard. "It must clearly have," he says, "very important relations to the blood"; whether accessory to digestion or having within itself the power of acting beneficially on the blood, he will not then consider. He then goes on to describe the fleshy hardness in some cases, and how in others the spleen reaches a prodigious size; this may occur even in young children, when it is usually accompanied by emaciation and often petechiæ on the body. There is also to be remembered the well-known enlargement which occurs in intermittent fever. It may also have growths in it like other organs.

Then he speaks of the enlarged or hypertrophied spleen, which is now recognised under the name of "leukæmia."

As regards the diagnosis, he refers mostly to the physical signs, as at that time no examination of the blood was made. He merely says that the edge of the spleen may be felt some way down the mesian side of the body.

The paper is accompanied by a plate, showing a diagram of the abdomen, and an outline of the

enlargement of the spleen reaching to the crest of the ilium.

He finally describes cases which are evidently the same as those which we now call "Hodgkin's disease," there being a doubt both with Bright and Hodgkin as to its cancerous nature. Bright gives great credit to his colleague for giving it a separate place in the museum.

He gives the case of a boy, aged 9 years, admitted into the hospital in October, 1826, very much emaciated from a seven months' illness. He very significantly states that he had always slept with his brother, who had just before died of phthisis. He had complained much of pain in the back, and on admission was found to have ascites. He died five weeks after admission. On post-mortem examination there was found extreme inflammation of the peritoneum, with copious scrofulous effusion. The mesenteric glands were greatly enlarged, and one or two equalled the size of a pigeon's egg, of semi-cartilaginous hardness. The liver had also a few tubercles larger than peas, and which were also of semi-cartilaginous hardness. The pancreas also contained similar bodies. The spleen was large, and contained white bodies of irregular ovoid shape. The opaque matter looked like tallow which had been melted, thrown into the tissue, and then cooled.

It seems to me that Bright, placing this case near those now called "Hodgkin's disease," regarded it as a form of it; or it might have been a case of what I had called "acute Hodgkin's disease," or of late named "status lymphaticus."

Vol. 4, 1839.—Addison contributes a paper "On the Disorders of the Brain Connected with Diseased Kidney." This seemed quite necessary as soon as Bright's disease was fully recognised and the symptoms connected with it; for at that time the most popular book connected with disease of the nervous system was that of Abercrombie. He, like previous authors, had spoken of serous apoplexy and congestive apoplexy; the first was so denoted to distinguish it from sanguineous apoplexy, which was caused by effusion of blood and generally accompanied by paralysis. But the symptoms which he mentioned under serous apoplexy clearly indicated disease of the kidney, such as the pale face, the small pulse, the pupils little affected, etc. All these symptoms, Addison said, denoted renal disease, and more especially what he called the quiet coma and regular breathing compared with the stertor of apoplexy; these he considered were the most characteristic symptoms of uræmic coma; although in some cases there might be convulsions, the examination of the urine generally would decide the question as to the symptoms being uræmic.

In this volume for 1839 Dr. Hughes writes on "Fibrinous Concretions in the Heart." He commences by saying that although Morgagni included this subject in his other contributions to morbid anatomy, the subject was in a very confused condition. In spite of what the distinguished French writers had done, there were still many difficulties as to what were to be considered ante-mortem clots or

concretions and those which were cadaveric. This paper considerably assists in clearing up some of these difficulties. The author shows that fibrinous matter which is adhesive, and especially to diseased surfaces, is deposited during life. It seems remarkable to us, who have more recent knowledge of the subject, that Dr. Hughes did not recognise the fact that some of this fibrinous matter might be washed off the valve and carried to distant parts, and so produce blocking of the blood-vessels. The facts which determined the doctrine of embolism did not clearly make themselves manifest until some time afterward, indeed, not until another fifteen years had elapsed, when Dr. Kirkes read his paper on the subject before the Royal Medical and Chirurgical Society in 1852. This was finally elaborated by Virchow and put on a firm basis. I take credit to myself for having furthered this end by adding one stage to it. Having observed that patients became affected much in the same way when particles of fibrinous matter were carried from the heart or larger vessels as they are when some deleterious matter is carried from the surface of the body, and which we call pyæmia, I called this other condition "arterial pyæmia," and wrote a paper under this appellation in the 'Guy's Hospital Reports.' When I say similar symptoms I allude to a certain amount of pyrexia and shivering being generally present. In a case of this kind Dr. Gull diagnosed it as ague and sent the patient to the sea-side; he knew that a mitral bruit existed, but it never occurred to him to associate this with the symptoms. I have dwelt

upon this because I have witnessed the whole development of this subject before it became quite manifest that we were looking upon a new form of malady, at least new and recognisable to ourselves.

To return to Dr. Hughes' paper, I will only give one interesting case bearing on this subject, but quoted from a French journal. "A woman suffering from symptoms of disease of the heart was suddenly seized with paralysis and loss of the pulse in the left arm. This rapidly became gangrenous and was the cause of her death. On post-mortem examination the brachial and ulnar arteries were found filled with fibrin of a yellowish-red colour, granular in texture, and adherent to the lining membrane of the vessel. A concretion of exactly the same colour, texture, and consistence was discovered in the left auricle and slightly adherent to its parietes and muscular columns. Then the author says that the fair and natural presumption is that both masses of fibrin assumed the solid form about the same time." I may add that the possibility never seemed to have occurred to him that they might be portions of the same mass of fibrin.

In this volume Dr. Bright contributes another paper on abdominal tumours, illustrated by "Diseases of the Kidney." This is followed by a very interesting paper by Mr. Towne, the wax modeller, "On the Incubation of the Egg," accompanied by some very beautiful coloured drawings. I will here quote his introduction in order to give a sample of the high scientific aim which our Treasurer had when he determined on the formation

of a school at Guy's Hospital. It was not only that good work should be done in the wards, but that scientific investigations bearing on physiological subjects should also be executed.

Mr. Towne commences his paper by saying that "some months have now elapsed since I received instructions from Mr. Harrison, the Treasurer of the Hospital, to prepare a series of models, illustrative of the changes which occur in the egg during incubation." The result of his experiments is now well known, but I may mention that they were sound, though tedious. It having been thought that the shell was pervious to air for the benefit of the chick, he varnished the egg and then placed several layers of paper over it, which he also varnished, so as to render the shell perfectly impervious. He then watched the process of incubation and found that his experiment had made no difference in the result. Then the question came, How did the ovum or cicatricula always appear at the upper part of the egg so as to be always immediately under the hen? This he found was the function of the "chalazes" which were attached to each side of the yolk and so kept the cicatricula floating in the midst of the white albumen.

Vol. 5, 1840.—This volume contains a paper by Dr. Golding Bird on the "Urine During Gestation." The subject having been discussed both in British and foreign journals, the author wished to make further chemical analysis, and more especially of the

pellicle which forms on the surface of the urine, and which is called "kiestine." The general belief was that during pregnancy milk was secreted, but being again taken up into the blood was thrown out by the kidney, producing the so-called kiestine. This was found by Dr. Golding Bird to be caseous matter, and that this change in the character of the urine appeared about the middle of pregnancy.

Then the volume contains a short paper by Mr. Wilkinson King, showing the action of the papillary muscles on the valves of the heart.

Dr. Bright also has a further paper on "Abdominal Tumours," describing the various diseases of the peritoneum. He relates a very marked case of colloid disease in a man, aged 44 years. There was found a large mass of semi-gelatinous growth in botryoidal forms involving all the viscera. As is usual in such cases he was unable to determine whether the disease began in the liver or omentum. Dr. Babington made a chemical analysis of the material, and stated that it contained no albumen but answered to all the tests of inspissated mucus.

Vol. 6, 1841.—In this, the sixth volume, there is a paper by Dr. Babington on "Chorea." In this, he says, "there is often a soft *bruit* accompanying the first sound of the heart," and in connection with this he continues, "out of a very large number of cases of chorea seen lately by my friend and colleague, Dr. Addison, to whom I am indebted for having first directed my attention to the point, only two have

been without a decided mitral or left ventricular bruit. In these two there was a diseased heart, and in one case, examined after death, there was found old thickening of the mitral valve, with very recent pericarditis. Should future investigations prove chorea to be more immediately dependent on disease of the heart or pericardium than has hitherto been supposed, the merit of discovery will certainly be due to Dr. Addison."

In this volume there is a paper on "Electricity as a Remedial Agent in the Treatment of Diseases" by Dr. Golding Bird. He first gives an account of the electrical room of the Hospital as being an apartment appropriated by Mr. Harrison, the treasurer, in the year 1836, for the purpose of submitting patients to electrical treatment. Amongst various diseases in which it was found especially successful was chorea, and also local forms of paralysis, especially those arising from lead poisoning; for example, that of a young coach-painter, who was unable to raise either hand, and who was quite cured by this means. In another case of dropped hand in a painter electricity was used with the most beneficial effects.

Dr. Hughes relates the case of a young man, aged 20 years, who was under his care at the Surrey Dispensary in December, 1840, with abdominal effusion, and when he was tapped the fluid which was drawn off was like milk. This, when analysed by Dr. Owen Rees, proved to be chyle. About six weeks after he died extremely wasted. On post-mortem examination there was found a large tumour, made up of conglomerated mesenteric glands, and

which was called tuberculosis, though it was uncertain whether it was not cancer; the lacteals over this were distended, and exuded the milky fluid. No doubt the thoracic duct was completely closed by the pressure.

Dr. Barlow has a paper, entitled, "Observations on Certain Diseases Originating in Early Youth." The first starting-point was deficient expansion of the lungs; this might arise from enlarged tonsils, some primary wasting of lung from a former pneumonia, or rickets; but, whatever was the cause, the relation between the chest organs was upset, so that, in the cases he mentions, the heart and the whole circulation are affected. This subject he further elaborated and illustrated in his Gulstonian lectures. He shows, if the expansion of the chest is in any way lessened, there arises, in consequence, deficient expansion of the lung; then the amount of blood is lessened, the mitral orifice (the measure of the whole circulation) is narrowed, as well as the pulmonary artery. His first case was that of a girl who came under his notice in 1837. She was 12 years of age, and suffering from great dyspnœa and feebleness. He kept her under notice for four years, when the difficulty of breathing increased, and death took place. On post-mortem examination, the chest was seen to be very narrow and contracted. The right side of the heart was considerably larger than the left, and the walls of the ventricle much thickened; the pulmonary artery was smaller than natural; the left auricle dilated and hypertrophied; the auriculo-ventricular opening only the

size of a shilling. Aorta smaller than natural, but otherwise healthy; trachea small. In speaking of the narrowing of the chest he mentions spinal curvature; and as regards obstruction of the throat, he probably referred to what is now called adenoids.

Another case was that of a lad, admitted into the Hospital in January, 1841. He had always been short-breathed, but of late had become much worse, and on admission he was quite livid. He shortly died. I need not mention the particulars of the post-mortem examination, as they resembled closely those of the last case.

Dr. Owen Rees and Mr. Lane gave "A Contribution towards the Structure of the Blood-Corpuscle." The microscope did not, however, determine with precision its nature.

Vol. 7, 1842.—In this volume Dr. Chevers has a paper on "Acute Aortitis and Disease of the Aortic Valves." I mention this more particularly for the purpose of giving a confirmation of what I have already stated*—that Dr. Hodgkin was the first who gave any scientific description of the phenomena attending destruction of the aortic valves. These are the words of Dr. Chevers: "Dr. Hodgkin was

* A paper in English (I think American), was lately shown to me in which I was severely criticised for making this statement about Hodgkin. Although the statement is true, I quite admit that the distinguished Irish physician, Dr. Corrigan, always had his name associated with aortic valve disease, in which Dr. Hodgkin had no special interest.

the first who described retroversion of the aortic valves. His paper on the subject, in the third volume of the 'Medical Gazette,' was one of the first essays in which regurgitation through the aortic valves was treated of as a distinct form of disease."

Vol. 1, 1843 (Series 2).—In this volume Dr. Chevers has an interesting paper on "The Cause of Death after Injuries and Surgical Operations in London Hospitals." It is very remarkable that these causes are not ascribed to blood-poisoning from the wound, which seemed so clear a fact a few years afterwards, and was demonstrated by the discovery of microbes by Pasteur. It was this which led to the wonderful antiseptic treatment by Lister. It was known that after a wound or injury, however slight, it might lead to the death of the patient. It was this fact which puzzled Dr. Chevers as well as all the surgeons who had lived before him. He then quotes many of these, and even Morgagni, who was appointed professor at Bologna University in the year 1715, and who said "that persons admitted into hospitals with slight affections and lay a long time, especially if they lay near sick, inhaling vitiated air, were subject to much more grievous diseases than those for which they were brought there"; and in illustration of this fact he cites the case of a man who entered a public hospital at Bologna for the cure of a slight wound in the leg, but lying next a patient suffering from an abscess in the thigh, which afterwards proved fatal, he

became the subject of acute fever, attended with cough (the sputa being tinged with blood and afterwards assuming a greenish hue), difficulty of breathing and pain on the right side of the chest, compelling him to rest on that side. He died on the fourth day of the attack, and it was found that his symptoms depended upon pneumonia. Dr. Chevers then quotes other examples from British and foreign literature.

Dupuytren had noticed the deposits of pus in the lungs as a result of injury; also Sir Charles Bell said that inflammation of the lungs was by far the most frequent cause of death in severe wounds and especially in compound fractures. Mr. Erichsen had found the same after injuries; other surgeons had noticed that after amputations, suppuration of the veins in the stump was accompanied by similar inflammation states in distant regions of the body. They were consequently inclined to attribute the symptoms to phlebitis.

Dr. Chevers says "he finds it necessary to speak of these facts because scarcely any allusion has been made to them in the larger proportion of surgical works, and yet every student who attends for any time the wards of a London hospital cannot fail to see that patients under treatment for the effects of common operations, however skilfully performed, compound fractures, slight punctures and lacerated wounds, are frequently after a transient fit of shivering, followed by an apparently slight febrile reaction, attacked with inflammation of some vital organ, without being accompanied by many of the

distinctive signs of disease which usually characterise severe inflammatory lesion of those parts in ordinary cases, yet destroy the patient within sixty or eighty hours from the commencement of the acute attack."

Dr. Chevers says it is evident that the inflammation of the external wound had not been propagated by continuation of structure, for these inflammations are found in the most distant parts; nor does he think some of the explanations are at all satisfactory, such as vitiated atmosphere or depressed state of the nervous system; nor is arteritis or phlebitis applicable, as their occurrence is rare.

Then, strangely enough, the writer says that "some surgeons have supposed that the pus has been absorbed from the wound or deposited in the lungs, but although it cannot be doubted that occasionally purulent and other extraneous fluids are carried into the circulation by the veins, there is no evidence of such venous absorption having occurred, so that such purulent deposits are evidently not mere filtrations."

I may remark that an essay like this is one of the most striking examples of the advance of medicine in my time, for although the microscope had not then been able to show the ultimate cause of this complaint, yet it is curious that a better generalisation from a clinical point of view should not have led to the correct conclusion. It must be remembered, however, that post-mortem examinations were then only exceptionally made.

Dr. Hughes gives a description of two cases of *glanders* which were under his care in the Hospital.

The first one was modelled by Mr. Towne in 1837 and is now in the museum. I might remark here that other cases subsequently occurred, and which were modelled to show the pustular eruption and the hard lumps which form all over the body. This is described in the 'Guy's Hospital Reports' for 1861.

A paper by Dr. Chevers on the "Coronary Arteries," their structure and function.

A paper by Dr. Addison on "Pneumonia." It enforces his argument, which he had always maintained, that the seat of the products of inflammation were in the air-cells themselves and not in an imaginary parenchyma between them. There is a very good drawing of lobular pneumonia with suppuration as a result of phlebitis after amputation of the leg.

Then there is a very good essay by Dr. Thomas Williams "On the Pathology of Cells." He gives an excellent account of the new doctrine of cells which the microscope had revealed — as their origin and growth with further development. He gave it as enunciated by the well-known physiologists, Schwann and Schleider, and as was being taught by all the best professors in Europe, such as Henle, Müller, Grainger, Bowman, Goodsir, Barry, and at Guy's by Gull in his introductory lecture on physiology, the first course of distinct physiology there given. This was the theory taught when I was a student and before the time of Virchow.

There is another paper by Dr. Williams (who died early) in a future volume on the simpler glands found in the lower animals.

Mr. Asten Key gives an account of his removal of an ovarian tumour from a girl, aged 19 years. She died five days afterwards of peritonitis. This was before the days of antiseptics.

In this volume for 1848 Dr. Lever has a paper on "Puerperal Convulsions," and finds that in many cases the urine was albuminous. In reference to this he says "the coincidence of an albuminuric condition of the urine in nine out of ten cases in which the secretion was examined is a fact, and, as far as my investigations and inquiries have extended, has not been previously remarked."

I believe this was the case, and credit has always been given to Dr. Lever as the first who observed albumen in the urine in cases of puerperal convulsions.

Vol. 2, 1844.—In this volume is a paper by Dr. Hughes and Mr. Cock on "Paracentesis Thoracis." It is remarkable that, in a common disorder like pleuritic effusion which everyone now can diagnose and then draw off the water by a simple operation, at the time which I am writing about when I was a student, a paper should have to be written by a physician and surgeon of the Hospital to point out the mode of ascertaining the presence of fluid in the chest and then the manner of drawing it off, together with a drawing of the trocar and cannula which were used.

Vol. 3, 1845.—This volume contains a "Report

of Cases of Diseases of Children treated at Guy's Hospital in 1843–44." Dr. Golding Bird, who writes it, begins by speaking of a ward set apart for children, which had been found very useful for teaching purposes, as well as of great advantage to the poor of the neighbourhood. Credit is given to Dr. Barlow for this important addition, sanctioned, of course, by the Treasurer. Dr. Bird says: "Some time ago my friend and colleague, Dr. Barlow, endeavoured to facilitate the labours of the student, as well as the administration of relief to the patients, by arranging the attendance of the infant applicants for relief at Guy's Hospital at the same time, and by having special letters given to them distinct from the adults. The plan met the approbation of the Treasurer, Mr. Harrison, who gave every facility for its being carried into effect, and thus on each out-patient day the students attending the medical practice of the hospital had an opportunity of seeing a large number of cases of children's diseases, to the investigation of which their attention could not fail to be peculiarly directed by observing them together unmixed with other cases. This was found to work well and efficiently for some time, when the charge of the children fell into my hands on Dr. Barlow giving up the care of the out-patients. A certain number of the cases were taken into the Hospital, where a special ward was provided by Mr. Harrison."

Then Dr. Golding Bird begins his report by saying that during the last year seventy-five cases were admitted and carefully recorded, and continues by

speaking of the gastric fever of children, and that the best remedy for the complaint was the "Pulvis rhei salinus" of the Guy's Pharmacopœia, given every morning for a week or longer. "This remedy," he states, "is well known to the profession for its peculiar and almost unique influence in these affections, so much so as, in my mind, almost to justify the elaborate praise accorded to it by Dr. Fordyce seventy years ago, when in his treatise on 'Fever' published in 1777, he says—'Had I been more ambitious of dying a rich man than of living a useful member of society, the power of our anti-hectic powder in curing, as if by miracle, the hectic fever and the swelled bellies of the children in this town would have remained a secret while I lived.'"

Dr. Golding Bird then speaks of croup. He seems to have a difficulty in understanding what is meant by the term; he mentions laryngismus stridulus and cynanche membranacea or the diptherite of Bretonneau. He advocates the usual method of giving an emetic of antimony. In his first case, a child aged 3 years, he gave half a drachm of antimony wine every two hours for a few doses. This made the child sick and he vomited some shreds of false membrane; at the same time he was profusely sweating. On the third day he was remarkably relieved; he progressed satisfactorily and then rapidly convalesced, leaving the hospital in less than a month from the time of admission. This is interesting in connection with the question afterwards discussed at the Royal Medical and Chirurgical Society whether the diphtheria, which

had lately come to the shores of England, was a new disease imported from the continent, or whether it was nothing more than the well-known membranous croup.

In this volume is an account by Dr. Oldham of a case of "Extra-uterine Fœtation." There is a good description of the case, accompanied by illustrative drawings, among which is the uterus with a most exuberant growth of deciduous membrane.

Vol. 4, 1846.—In this volume is a further paper by Dr. Thomas Williams on the "Physiology of Cells." It has already been referred to in a previous volume.

There is a paper by Mr. Wilkinson King on the "Appearances in the Stomach after Death." He begins by saying that in spite of John Hunter's description of the digested stomach in man and animals, the appearances found after death, and especially perforation, are constantly referred to the effect of some irritant poison. In consequence of this he enters into fuller particulars of the appearance found after death.

Dr. Hughes gives a "Digest of a Hundred Cases of Chorea treated in the Hospital."

Vol. 5, 1847.—This volume contains a very interesting paper by Dr. Lever on the "Disorders of the Nervous System associated with Pregnancy and Parturition."

Then follows "A Rare Case in Midwifery," by

Dr. Oldham. This was a case of a woman, aged 40 years, who had had several children, when, being again pregnant, she engaged a medical man to attend her at the expected time. No labour came off and all the usual symptoms of delivery passed off. Some time afterwards a dead fœtus was expelled in various portions and was much decomposed. Dr. Oldham considered that the case was so different from others which he had seen of extra-uterine fœtation that he considered that for some unknown reason the uterus had lost its tone, and so no delivery took place and as a consequence the unborn child died. In Dr. Oldham's own words, "The foregoing case is one of great rarity in midwifery. It appears to have been an instance where a female carries a child in the womb to the full period of gestation; but the process of labour is literally *missed* and lactation follows on the completion of gestation. The womb then remains passive and incapable of being excited to act. It holds the fœtus like an extra-uterine cyst, shrinking, however, gradually with the lessening of the contents." The patient, owing to the continual discharge, gradually wasted and died. A post-mortem examination was made, and the parts involved were removed and preserved in the museum of the hospital. The editor of the 'Guy's Hospital Reports' adds that some years afterwards the specimen was examined by Dr. Galabin, who stated that it was clearly a uterine interstitial pregnancy at the commencement of the Fallopian tube, and so the cyst formed around it opened directly into the uterus.

In this volume is published the "Annual Report of the Clinical Society." Amongst the cases it will be well to mention a case of hemiplegia treated by means of a local production of galvanism. The patient was a tanner, aged 32 years, and was admitted for hemiplegia which occurred without any assignable cause. The paralysis was on the right side and implicated both sensation and motion. Two blisters having been applied to the arm, one over the insertion of the deltoid, the other at the posterior part of the wrist-joint, a zinc plate with a copper wire attached was placed on the upper denuded surface and a plate of silver over the lower. On the wires being connected a tingling sensation was produced. In a short time the upper sore was coated with a layer of lymph. At the end of a week the apparatus was removed; the lower sore had healed, while a slough, about the size of a half-crown piece, occupied the upper or zinc sore. The slough soon separated, and left healthy granulations. The patient left the hospital, having regained the use of his arm.

Vol. 6, Part I, 1848.—This volume contains a highly scientific paper by Dr. Thomas Williams, whose early death was a great loss to scientific medicine, "On the Distribution of Life in the Sea and Atmosphere" or the laws of "Aquatic Breathing."

This is followed by a short paper, contributed by Dr. Alfred Taylor, on the "Alleged Production of Phosphate of Lime and Iron in the Egg during Incubation." It had been thought by many persons,

and absolutely stated by some eminent men, especially by Sir Gilbert Blane, that many substances were created as iron and lime in eggs. Taylor showed how these substances existed in the chick, more especially in the feathers, and by the same tests both iron and lime could be found in the albumen of the egg.

Vol. 6, Part II, 1849.—Mr. Bransby Cooper writes a paper, entitled, "Considerations connected with the Pathology and Surgery of Urinary Concretions," with some beautiful coloured drawings of sections of the different kinds of calculi.

Mr. Birkett has a paper on "Adenoid Disease of the Breast," and mentions the various names which tumours of this kind had formerly received, that is, before their nature had been made out by himself to be of an imperfect glandular structure resembling that of the breast itself. Sir Astley Cooper had called the disease "cellular hydatids," Sir Charles Bell, "hydatic carcinoma of the breast," Sir Benjamin Brodie, "sero-cystic sarcoma," and there were several other names.

Vol. 7, Part I, 1850.—In this volume is a communication by Mr. Bransby Cooper on the use of the microscope. It may seem remarkable to the student of the present day that such an essay was necessary only a few years ago, but he should remember that it was to the microscope we owe the modern

advancement of medicine. It has enable us to look into another world, a world altogether unknown a few years ago, as it was not visible to the naked eye. All now is altered, and most of our treatment and diagnosis is due to this instrument. Before its use very little was known of the structure of the various organs of the body. I will therefore quote the beginning of Mr. Bransby Cooper's preface, from which it will be seen that in his time we were in the infancy of microscopic examination. He begins by saying: "It is but a few years since the microscope was employed for no other purpose than mere amusement, and a man of science, more especially if he were engaged in the study of medicine, scarcely dared to admit that he sought its aid, for fear of having it said that he was engaged in investigations quite foreign to his legitimate pursuits, and that he was trifling away his time. Lately, however, the microscope has become more justly appreciated, and is now very generally employed as an accessory in the study of general anatomy and pathology. I confess it was with difficulty that I could bring my mind to believe that the investigation of the molecular structure of the tissues should ever tend to the advancement of medical science, and it required a struggle to overcome the '*vis inertiæ*' of my mind and the stubbornness of ignorance before I was induced to examine the minute structures of the various tissues of the human body by the aid of the microscope. I determined, however, to overcome my prejudice, and in a very short period I became impressed with the thorough con-

viction of the utility of the microscope in pathological investigations. By the assistance of this instrument, I, for the first time, began to understand that the physical and vital conditions which render the various tissues of the body competent to fulfil their important offices in the animal economy depend in great measure on the ultimate arrangement of their proximate elements, a knowledge which could never be acquired by the minutest dissection unless aided by the microscope. As soon as the structures of the body in a state of health had become by practice easily recognisable under the microscope, and I was able to distinguish the smallest portion of bone, cartilage, muscle, or fat, etc., from each other in consequence of their unvarying structural character in the normal condition, I was soon incited to examine microscopically the same tissues when affected by disease, and I found the deviations from their natural organisation were as appreciable as their normal structure, and I am now convinced that the microscope is as necessary to the anatomist and pathologist as the scalpel to the one and the bedside observations to the other."

Mr. Bransby Cooper then refers to Mr. Quekett, who was one of the first to use the microscope, and therefore became much consulted in cases where it was thought it might be useful, and quotes one or two instances. For example, in the case of a young man, aged about 16 years, who had become very thin and at the same time the subject of a frequent cough, some of the sputa was sent to Mr. Quekett, who on examination found not only pus, but small

portions of the fibrous structure of the lung. He predicted consumption, and a few weeks confirmed the truth of the suspicion.

Then the author refers to the case of a young lady who had a tumour on her back, and from this he drew off some fluid by means of a trocar. On examination of this under the microscope he found echinococci, and therefore at once pronounced it to be an hydatid. Mr. Bransby Cooper mentions that she got quite well, and "she told me that her face and neck became covered by an eruption on the day I drew off the fluid and remained nearly a week and then disappeared." This is the first time I have met with this roseolous or urticarious eruption being noticed as occurring after puncture of hydatids, a fact now generally known.

He further says that he hopes there may be found some special virus which will greatly aid in the treatment of syphilis. His friend Dr. Donnet, R.N., believes that he has ascertained that the specific virus contains vibriones, but he, Mr. Barnsby Cooper, says this requires corroboration, but still the microscope should be used for such examination.

In this volume Dr. Hughes has a paper on "Anæmic Murmurs and their Diagnosis."

Also Mr. Hilton on "Dividing the Lingual-gustatory Nerve in Cancer of the Tongue." This was for the purpose of destroying the sensibility of the ulcer so as to enable the patient to take food and to reduce the flow of saliva. It may allow also the use of the ligature to produce sloughing of the diseased portion from the healthy. In illustration

he gives the case of a woman, aged 39 years, admitted under his care in February, 1850. He describes how the tongue was held forward, and a cut by the side of it opposite the molar teeth exposed the nerve. This was then cut through with scissors. He afterwards removed a large part of the tongue by ligature without the patient feeling any pain.

Vol. 7, Part II, 1851.—In this volume is an original paper by Dr. Addison and Dr. Gull "On a Certain Affection of the Skin, Vitiligoidea plana and Tuberosa, with Remarks." This is accompanied by coloured drawings showing the eruption on the face and other parts of the body.

Vol. 8, Part I, 1852.—In this number is a paper by Dr. Barlow "On the Pulse," giving an account of its different characters dependent upon the heart, and the circulation through the arteries, veins, and capillaries.

A further paper by Dr. Gull on "Vitiligoidea."

An account of "Follicular Disease" by Mr. Cock.

Then a paper by Mr. Hilton on "Intestinal Obstruction relieved by Operation." He gives a detailed description of colotomy, which was then only coming into practice, as he shows there had only been a few cases where the operation had been done.

Vol. 8, Part II, 1853.—This volume contains the first paper written on "Diabetes" by Dr. Pavy, who subsequently contributed several others to the 'Reports.' Having studied in Paris under Claude Bernard, he interested himself in the production of sugar and diabetes. He repeated Bernard's experiment on a rabbit and dog in the laboratory of Guy's by puncturing the fourth ventricle. This he records in the present volume with drawings of the instruments used and the fourth ventricle exposed.

Then follows a paper by Mr. Salter "On Dentine of Repair, and the Laws which regulate its Formation." This is illustrated by cases showing how a tooth is repaired. Then follow some highly scientific lectures by Mr. Hilton "On the Development and Design of Certain Portions of the Cranium." He speaks of the smoothness of the interior of the skull compared with the exterior, showing various hollows and prominences having distinct purposes; then the hard ridges for muscular attachment and others following the lines of mechanical force, especially for the support of the head. Then he refers to the movement of the jaws in connection with their muscular action.

Then follows the "Clinical Report" for the year 1853 by Samuel Wilks, M.D. In this report I mention the case of a woman, aged 26 years, admitted into the Hospital for "fits." These gradually increased in frequency, and then, with other brain symptoms ensuing, she died after a few weeks. The interest of the case was owing to the fact that, during the occurrence of the fits, she never lost her

consciousness; it was thought, therefore, that they were due to a local brain disease rather than ordinary epilepsy, for Bright had already taught that it was clear in the latter case the whole brain for the time was affected, whereas in a local disease this would not be the case. There was, therefore, a tangible disease causing the fits, and in this patient some tumours were found in the hemisphere.

Now I will remind the reader of Bright's words in the first volume of these 'Reports.' He there states: "My reason for supposing that the epileptiform attacks in this case depended rather on a local affection than on a more general state of the cerebral circulation and excitement was the degree of consciousness which was observed to be retained during the fits." This experience he said he had obtained by the experience of two previous cases.

I give in this report a fatal case of hæmoptysis in a man, aged 33 years. He had been the subject of severe hæmoptysis for six years, but in the intervals of the attacks he had been well enough to follow his employment; an attack more severe than usual brought him to the hospital, and before long a large gush of blood took place which was fatal. The post-mortem examination revealed a number of cavities formed by dilatation of the bronchial tubes, and into one of these a branch of the pulmonary artery opened. I mention this case because it was the first which I had met with of fatal hæmorrhage in phthisis, but I found that in all subsequent cases they were of the same character—that is, that they were chronic, and never displaying any severe symptoms apart

from the hæmorrhage, which varied very much as to frequency; but invariably there was found a blood-vessel which opened into a smooth-walled cavity, or not infrequently the ruptured vessel was the pouch of an aneurysm.

In this report are four cases of cæcitis which were all cured. This term has been changed for typhlitis or perityphlitis, and in more recent years for appendicitis. This designation, besides being uncouth, is eminently bad, because it is always assumed that every inflammation or abscess in the neighbourhood of the cæcum is due to disease of the appendix. No better illustration could be given of this than the case of his late Majesty King Edward. In his case the surgeon had not long before made the statement that it was only about fifteen years before that time that appendicitis had been recognised. Now, as a matter of fact, Dr. Addison had lectured upon it for many years, and in his published lectures for 1836 the account which he gives of it is as good and excellent as any to be found in the modern treatises. He uses the word "perityphlitis" and describes its symptoms and treatment; and in this account he states that the most usual cause of the disease is a calculus in the appendix. Showing how preferable the term which he gives is to that of appendicitis is the fact that the surgeon, finding that the King had an abscess and no trace of appendix, was forced to use the term "typhlitis" in his bulletin. The appendix might, however, have been originally the source of the trouble.

It is now well known that syphilis may seriously

affect the arteries and is therefore one of the causes of aneurysm. Besides hospital cases I had three friends who died young from aneurysm of the aorta and they all had constitutional syphilis. The earliest case where I observed it is mentioned in the "Clinical Medical Report" for 1853, from which I have been quoting. It was the case of a woman, a prostitute, who had suffered very much from syphilis and pains in the limbs. On the day after admission she was getting out of bed when she suddenly fell back and died. A large abdominal aneurysm was found between the cœliac axis and the iliac bifurcation which had ruptured.

Then I mention under skin diseases a case of "keloide" by Addison. I may say that Addison took a special interest in skin diseases, and was even a great authority on the subject before it was made a speciality; this occurred most probably from his having been a colleague of Bateman at the Royal City Dispensary, the latter having been joint author of the standard work on 'Diseases of the Skin' by Willan and Bateman. Addison described this keloide or cheloid disease as having two forms. The one showed itself in the form of livid red lumps; the other resembled more the cicatrix of a burn, the skin being seamed, raised, and contracted. The cases which Addison described were in two young girls, the one having round, red excrescences on the chest, in the form generally mentioned by authors; the other girl had a scar like a burn all down the front part of the right thigh in a form which had not yet been fully described. Alibert,

the great French authority, called this disease first mentioned "*kéloide*" from κηλις, a brand or scar. The word is sometimes spelled "cheloid" in reference to the "claw" appearance of the disease as if from χηλη; while others who have represented the tortoise-like shape it has assumed would refer it to χελυς.

Another case which is mentioned in the "Report of the Clinical Society" is one which Addison had under his care for a very long time because it resembled the leprosy of the East in every particular. The patient was a tailor and an Irishman and had never been out of Great Britain. When first he came under notice he said the complaint had been coming on for three years. He first felt pricking and numbness in the hands; at the same time his voice became husky. His hands and feet were much altered in shape, the skin thickened and covered with fissures and ulcers. His face presented a most hideous appearance; his lips were thickened and his nose flattened; there were large folds of skin overhanging the eyes and these nearly met two other large masses of skin coming from the cheeks on both sides. He died eventually from ulceration of the windpipe extending from the vocal cords to the bronchial bifurcation. Addison called the disease the elephantiasis of the Greeks or the French term "*lépre tuberculeuse.*"

In this report I also mention, in connection with apoplexy and paralysis, the state of the pupil, in which at that time I was much interested. I state that in two cases of right hemiplegia the left pupil

was dilated, and in three cases of left hemiplegia the right pupil was dilated.

When first I went to Guy's Dr. Bright had just retired from the hospital, but I knew his name well from its being given to the malady which was called "Bright's Disease." I consequently took a very special interest in it, and so I naturally looked for cases in the wards. Of these I collected a considerable number, and, having learned all that was written and known of the subject, I put them together, with my comments upon them as an essay, to compete for the prize given by the Physical Society. I was naturally delighted when the prize was awarded to me, and the books I received with the award I still cherish as some of the dearest on my shelves. They are dated April, 1845.

In this volume for 1853 is the revised essay on "Bright's Disease" for which I gained the prize in 1845 of the "Pupils' Physical Society." It contained, I believe, some original statements of my own, but for the most part was an epitome or reflection of the opinions of the staff at Guy's, some of the physicians having been colleagues of Dr. Bright. Their opinions seemed to me somewhat different from those which appeared in foreign works as enunciated by French and German physicians. I might here mention as a point of historical interest the mode in which albumen was in my time shown to exist in the urine. The suspected water was placed in an iron spoon and the tip of it placed over a candle. If albumen was present the water became opaque and white streaks of it passed backwards into the

fluid. The test was very good, and those who used it preferred it to the new one of a glass tube, where the water became opaque as a whole but not in streaks, which made it distinguishable from the opacity or precipitate of phosphates. About this time albuminuria became the synonym for Bright's disease.

The most important fact in connection with the discovery of albumen in the urine was one which Bright himself in his original work did not lay so much stress upon (at least in his own writing) as the difference between the kidneys themselves, which he delineated in coloured plates. But in my time I found a great distinction made between the acute and chronic forms of kidney disease both as regards their appearance after death as well as the symptoms during life. In acute cases the water was discoloured, was albuminous, and the patient had dropsy, and after death the kidneys would be found large and pale, whereas in the chronic cases there was no definite history of a commencement, there might be no dropsy, and after death the kidney would be found shrunken and granular. Opposed to this some continental writers maintained, as was said to be the case in many other complaints, that the disease began as an acute affection and then became chronic. The first physician I believe who added anything material to our knowledge of Bright's discovery was Dr. George Johnson, who found casts of the renal tubules in the urine which might be seen easily by the microscope. From this fact he called the disease "desquamative nephritis."

I may further mention that some years afterwards Mr. William Gull associated himself with Dr. Sutton, both of whom had long been interested in the state of the blood-vessels in Bright's disease, and they came to the conclusion that the change in the blood-vessels and thickening of the arteries was the marked pathological change in the chronic form of Bright's disease, and they proposed that the name should be altered from that of "Bright's disease" or "granular kidney" to "arterio-fibro-sclerosis."

As regards the first contention, that the acute form of the disease was quite distinct from the chronic form, both as regards the symptoms as well as that of the organ itself, the one being enlarged and pale, the other of natural red colour but red and contracted, this seemed to be the opinion of the physicians when I entered upon the investigation of the subject; but it seemed never to have been put on paper or published before the appearance of my essay in the 'Reports,' and it was this distinction which I emphasised. In reference to this point, I may be allowed to state that some years afterwards I came across the article on "Bright's Disease" in 'Ziemssen's Cyclopedia of Medicine,' written by Professor Bartels, of Kiel. When speaking of the paper on "Bright's Disease," by S. Wilks, M.D., in 'Guy's Hospital Reports,' 1853, he says: "Samuel Wilks was the first to prove, and he did so in the clearest possible manner, from the ample clinical and pathological materials at his command, that the condition of the kidney of which we are

now talking, and which he described as 'the large white kidney,' ought not to be regarded as the precursory stage of that atrophic process which the German pathologist had proclaimed as the ultimate stage of every diffuse inflammation of the kidneys. That is the third stage of their Morbus Brightii.

"In Germany the work of Samuel Wilks seems to have received little or no notice or recognition. At all events, it exercised no influence upon the descriptions of the diffuse renal disease that appeared in our handbooks of clinical medicine."

The second series of reports just closed was edited by Dr. Birkett, who then resigned his office, being under the impression that the object of the work had been fulfilled, especially (as he said) as the weekly journals had taken its place, and essays on various subjects were continually read and afterwards published by the different societies.

At a meeting, however, of the professors and teachers of Guy's another opinion prevailed, and it was proposed to continue the 'Reports' as before. As arrangements could not be made immediately, one year was omitted, but a volume was promised in October of the following year—1854—and Mr. Poland and myself were appointed joint editors.

As already observed, with regard to post-mortem examinations, as they had never previously been performed systematically, there was still a large field of material unworked and an opportunity of discovering many morbid changes in organs which had hitherto never been described. These, therefore, I shall more especially select from the 'Reports' to place amongst the subjects of greatest interest.

Vol. 1, 1855 (Vol. 16 from the beginning, Series 3).—The first paper in the volume, by Dr. Gull (as he was then called), though short, is exactly of that kind which is most suitable for a hospital report. It is entitled "Notes on Tænia, with Fifty Cases treated by the Oil of Male-Fern." He mentions that since the introduction of this remedy he and his colleagues must have given it in nearly 200 cases, but he himself had carefully watched only fifty cases so as to be sure of the right diagnosis and result. The only perfect test of the whole worm coming away is the discovery of the head, and for this purpose he used to draw this head with four suckers so that it could be more easily recognised by ignorant people. He was quite satisfied that the male-fern was the best remedy we possessed. He mentions that the term "male-fern" is a misnomer, as it has no botanical meaning, and the so-called oil is an inspissated etherial tincture of the rhizome of *Aspidium filix-mas*. The author also says that the proof of the value of the remedy is seen by its universal use in Abyssinia and Nubia, and it is remarkable also for being no new remedy, for it is highly lauded by Theophrastus in his 'History of Plants' nineteen centuries ago.

Dr. Pavy has another paper on the "Formation and Destruction of Sugar," as at first described by Claude Bernard. This question Dr. Pavy had long studied, that is, the manufacture of sugar in the liver and then its destruction in the various processes of animal life.

Dr. Habershon takes up the subject, to which

Dr. Handfield Jones had drawn attention: that of studying the microscopic changes which the mucous membrane of the stomach undergoes in various morbid conditions. Dr. Habershon made several very good drawings of these morbid states, such as atrophy and other changes in the follicles. Some of the drawings show sections of the walls of the stomach where the gastric follicles are better seen in a variety of affections.

Mr. John Birkett has a very elaborate paper on "Adenocele," or, as we should now say, adenoid growths in the breast, the term not now being confined to the mammary gland. It is very interesting and to a great extent original. He commences by giving the older names for these more innocent tumours, such as chronic mammary tumour, cysto-sarcoma, fibrous tumour, glandular tumour, proliferous cysts. He now places them all under the epithet glandular tumours or adenoid tumours, and shows how the varieties differ according to the more or less perfect development of the new glandular tissue. Some showed merely the termination of the cæcal ducts; then there were other forms of tumour where the ducts themselves were also present, and so very closely resembled the original healthy tissue. I believe Mr. Birkett obtained the prize given by the College of Surgeons for the best essay on diseases of the breast, and I believe this laid the foundation for his larger work on the subject.

I have a further contribution on the subject of fever, to prove more absolutely the distinction between typhus and typhoid, as Jenner had shown

some time before. It took a long time to place all the facts, showing their difference, on a true basis. I was one of the first of the younger physicians to accept Jenner's views, who had been brought up in a different school.

I have also a short paper on "Leucocythæmia," a term only just come into use when any excess of white globules was found in the blood. This was in the year 1852, and at that time the blood was frequently examined in order to determine the difference in amount between the red and white corpuscles. It was contended by some good observers that these white cells were really pus-cells which had been formed, and then entered the blood. I, with others, examined the blood in a great many cases, and especially in anæmic cases, but in these I found no excess of white; the red corpuscles were not then counted. I, however, then confirmed the statement first made (I believe) by Dr. Hughes Bennett, that in great hypertrophy of the spleen there was a large excess of white corpuscles. Nor did I find any greater number of the white cells in the idiopathic anæmia of Addison any more than I did in simple anæmia; in fact, after examining more than a hundred cases where I thought it likely there might be an excess, I found only two—the one with the enlarged spleen, and the other in the case of a man dying of typhus fever, where a very large excess of white corpuscles was found.

There is a short paper by Dr. Gull on "Fatty Stools from Disease of the Mesenteric Glands." His object was to teach that the stools should be more

carefully examined in children, which was exemplified by a case under his care of a little girl whose bowels were frequently moved, and the stools frothed like soap when a stream of water was poured upon them. He then put them to a further examination, and found that they contained a quantity of fatty matter; this turned out to be nothing else than the cod-liver oil which the child was taking. He inferred from this that in a case where the mesenteric glands were tuberculous absorption of fat could not take place, and thus the extreme emaciation was accounted for. After the child's death he found all the mesenteric glands diseased just as he had expected.

What he had said was by no means contradicted by the observations of Bright, who had found fatty stools in persons, which was evidently due to the fat not being assimilated, as the patients evidently had some disturbance of the digestive system. He, himself, from the observation of several cases, believed it was due to disease of the pancreas.

Vol. 2, 1856.—The first subject in this volume is a case of "Myeloid Tumour of the Scapula." The case was that of a young woman who had a tumour growing from the left scapula. As it continued to grow it was thought advisable to remove it. It sprung from the spine of the scapula, had bony walls, but the interior consisted of a soft, pinkish substance, which under the microscope showed large polynucleated cells. It evidently belonged to the

class of growth which Paget called "myeloid," meaning it resembled the marrow of bones, even if it did not actually have its origin in the centre of a bone, as in this case, where it grew from the spine of the scapula. The pink colour is well shown in the drawing. The cells very much resemble the harder growths of the gum, usually called "epulis."

"The Third Septennial Report of the Lying-in Charity by the Obstetric Physicians" contains an account of several cases of puerperal convulsions. In all the urine was albuminous, and out of ten cases five died. The report states that the disease varied much in number at different times. During the seven years of this report there had been ten cases, but they all occurred during the first four years, and none during the last three. Again, there were ten cases in all during the last seven years, but in the previous septennial period there were only five.

Dr. Hughes has a paper on "Coloured Urine." He had a patient, to whom he was giving a drop of creosote every four hours. Three days after the commencement of its administration he observed that the urine was black. He had previously known nothing of its occurrence, but on looking up the subject he found that black urine had already been noticed by Dr. Marcet in the year 1814; but at that time creosote had not been discovered. His account of it is to be found in the twelfth volume of the 'Medico-Chirurgical Transactions." Marcet also alludes to a case he had under his care at the City Dispensary in the year 1802.

Then follows a long paper by myself entitled "Cases of Lardaceous Disease and some Allied Affections." I begin by stating that this form of disease of the liver is frequently overlooked, and as regards a similar condition of the spleen and kidney it is altogether unknown to many in the profession. (It must be remembered that this paper was written in the year 1855, which is now more than fifty years ago.) As regards the best name for this morbid condition several have been proposed, but the word "lardaceous" has become the usual expression and I shall continue to use it. When first recognised it was supposed to have some connection with tubercle, because so frequently met with in cases of phthisis; this seemed to be supported by Dr. Budd, who, in his treatise on "Diseases of the Liver," stated that he had mostly met with lardaceous disease of this organ in cases of affections of the bones or in long-standing disease of the hip-joint. As these were styled scrofulous and this term was almost identical with tuberculous, there was a further reason for associating it with these diseases.

I determined, therefore, in order if possible to discover the causes and nature of this affection, to collect a number of cases where it occurred in any of the organs of the body, and see if there was any common ground amongst them which might assist us in the discovery of its causation. Out of 36 cases of lardaceous viscera 16 had necrosed bone and 11 very evident disease of a syphilitic or other affection of the osseous system, leaving only 9 where no history of disease of this kind existed. In these

last cases some were associated with phthisis, leaving only two, or perhaps three, where lardaceous disease appeared to be primary. In these the kidneys were affected throughout their whole structure, and there being also dropsy and albuminous urine it was thought most probable that this might be one form of renal disease producing all the usual symptoms, but known only after death.

Nine years afterwards I published another paper on lardaceous disease in the 'Reports' (vol. xi for 1865), where I gave sixty more cases, which, added to these, makes ninety-six in all, and in reference to the whole I say that "the disease implies a long-standing cachexia, which is seen in its most intense form after a protracted caries and necrosis of the bone, which has had its origin in tuberculosis or syphilis; only in a few cases can the existence of either of these not be shown. With the exception of these, it may be said that in a very large majority of cases of lardaceous disease there may always be found a history of bone affection, syphilis, or scrofula." It will be seen that the title of the paper is "Lardaceous Disease and some Allied Affections." One of these is what is now called Hodgkin's disease, which at that time had not received this appellation or any other distinctive name.

When it was subsequently found that it was this disease which Hodgkin intended to describe in the paper which he read at the Royal Medical and Chirurgical Society, and published in its 'Transactions' for 1832, entitled, "On Some Morbid Appearances of the Absorbent Glands

and Spleen," I carefully perused it. After describing the enlarged spleen, and the deposit like suet scattered through it, he continues by saying that "the morbid alterations of structure which I am about to describe are probably familiar to many morbid anatomists, since they can scarcely have failed to have fallen under their observation in the course of cadaveric inspection. They have not, as far as I am aware, been made the subject of special attention, on which account I am induced to bring forward a few cases in which they have occurred to myself."

It is quite true, as Hodgkin states, that such a marked form of disease could not possibly have been overlooked, and certainly not by anyone who was daily making post-mortem examinations. Therefore, before very long an example came before me at my hospital, and I showed the specimen at the Pathological Society in the year 1865. The disease was so little known that I was credited with having made an original observation, and this was stated in the medical journals, and also in those published in America. What followed afterwards I will not give in my own words, but in those of a distinguished Physician of St. Bartholomew's Hospital. In speaking of the discovery of this disease being attributed to myself, he adds that: "Dr. Wilks, with the generous desire to perpetuate the name of his predecessor in the office of Teacher of Pathological Anatomy at Guy's Hospital, gave the name to this morbid state of 'Hodgkin's disease.'"

Dr. Gull gives cases of "Paraplegia," and speaks of arachnitis, softening, and cases of tumour.

This is followed by another paper on "Pityriasis Versicolor." This cutaneous disease has been regarded by Robin as parasitical but denied by Erasmus Wilson. Dr. Gull thoroughly investigated the question and found in all cases the presence of a parasite, and in one case which he describes it was in its most perfect form. This is shown in a most excellent woodcut, where the mycelium is seen and some of its branches have receptacles with sporules.

Dr. Habershon has a paper on the "Abdominal Sympathetic Nerve and on its Union with the Phrenic and Pneumogastric Nerves," and then shows by different illustrations how many symptoms of abdominal disease may thus be accounted for.

Dr. Pavy has a paper entitled "On the Gastric Juice as a Solvent of the Tissues of Living Animals." His paper gives an account of his experiments accompanied by drawings, which show a rabbit's ear and frog's leg which have been eaten away or dissolved in the same way as food or any other dead substance, by being put through a tube or opening in a dog's stomach.

Vol. 3, 1857.—I have a paper on the various diseases of bone, such as osteoid cancer, osteosarcoma, myeloid, etc. I discuss the question how far we must study the character of these different forms of disease, as it seems that they may be regarded as "malignant" when spreading through the body, but at the same time they might be called

"benignant," as they put on the character of the tissue whence they arise. Thus it is difficult to see how any special element or cell can exist in cancer which can be called specific and comparable with that which we call specific when we speak of syphilitic or tubercular disease. These run a definite course accompanied by similar phenomena, as in phthisis, where tubercle may be found in any part of the body, but it is not very clear how this could be done in these diseases having different forms, dependent very much upon the character of the tissue in which this grows.

I might even say that when the microscope was first used for purposes of diagnosis no particular characteristics of its cells could be discovered, although many different forms were thought to exist; nor since that time have any bacteria or distinct bacilli been found. I should say that at that time it was supposed that a characteristic cell *must* be found in cancer, which when found differed from others, and yet these must have produced it. I imagine they must be actually the same kind of cells, but for some reason did not develop into the tissues but remained in their primitive condition, and for this reason I called them *parasitic* cells. This opinion was strengthened by observing that a certain amount of natural growth of tissue generally took place at the same time, so that if a growth took place in the choroid of the eye black pigment was produced, and the same if the growth occurred on a nævus where pigment existed. The same might be seen elsewhere: if a growth took place from a bone it became

osseous; if it grew from the interior or marrow it was myeloid; if an epithelial cancer occurred in the rectum it was epithelial in character and of the same structure as the bowel itself. Now, further, it must be observed that all these growths are called malignant because supposed to arise from some vice or cause in the whole of the system, and they all partake of the same character as the original growth, which appears to depend upon their source.

Dr. Gull follows with an article on "Cerebral Abscess." He commences by first taking those cases associated with disease of the internal ear; then those where there had been a history of injury, leaving others where there was nothing to indicate their existence by any symptoms of an especial cerebral character; in two cases the matter was contained in a cyst and evidently old; in some others they possibly might have been pyæmic.

Then follows a paper by myself on "Idiopathic Fatty Degeneration." This term I used as it was the one long adopted by Dr. Addison in those cases, usually fatal, where no cause could be found for the anæmia. I will quote now his own words: "For a long period I have from time to time met with a very remarkable form of general anæmia, occurring without any discoverable cause whatever—cases in which there had been no previous loss of blood, no exhausting diarrhœa, no chlorosis, no purpura, splenic miasmata, glandular, strumous or malignant disease. Accordingly, in speaking of this form of anæmia in clinical lecture, I, perhaps with little propriety, applied to it the term *idiopathic*, to dis-

tinguish it from cases in which there existed more or less evidence of some of the usual causes or concomitants of the anæmic state."

In the heart of the fatal case there was seen a series of white lines or zigzag markings running across the columnæ carneæ of the mitral valve. These under the microscope showed distinct rows of fat-globules taking the place of the muscular tissue; judging from their width, at least half of the papillary muscle must have undergone degeneration.

In connection with this paper I alluded to some remarkable cases of enlarged lymphatic glands and deposit in the spleen. These, with some already mentioned in the last volume, are the same as were subsequently known as Hodgkin's disease.

I follow by a description of some wax models of skin diseases which had recently been made by Mr. Towne, our artist. I might say that they had been exhibited at the Paris Exhibition the year before, and gained for him a medal given in the Fine Art Department. These models were called "Roseola cholerica" and "Roseola variolosa." The first-named rash had often been observed to accompany the secondary fever of cholera in all the epidemics which have occurred in Europe. It is something like "Urticaria febrilis," covering the whole body and limbs with large raised, bright pink patches, more rarely like measles or scarlatina. We had 112 cases in our cholera wards at Guy's in 1857; of these more than half died, and of those who recovered five had this eruption, and these were the

patients who were copied in wax. I have noted that the first mention of this rash was by Dr. Keir, who observed it at Moscow in 1831. It was also described by Rayer as occurring in Paris in the epidemic of 1832, and he states that it resembled scarlatina. In the same year Dr. Babington referred to it in the 'Medical Gazette,' where he described the outbreak of cholera in Russia, there called "cholera spasmodica." It was described also by some of the German physicians during the epidemic of 1849.

Vol. 4, 1858.—This volume contains a case of opening the stomach in a patient who had stricture of the œsophagus, and as, I believe, it was the first case in this country where this operation was performed, it is generally alluded to in surgical works in connection with gastrotomy. It had been first suggested by Dr. Habershon, who considered it quite feasible in consequence of there having been many cases recorded where accidental wounds of the stomach had never healed, but remained open as permanent fistulæ for many years. Seeing no objection, he intended to recommend it at the first opportunity. The cases he thought of were stricture of the œsophagus where the tube became closed, but as most of these were malignant he expected no more than a prolongation of life. Soon afterwards the opportunity occurred. A man entered the hospital aged 47 years, who had

had difficulty of swallowing for a considerable time. On examination there was found a growth at the upper part of the œsphagus which almost closed the passage. So an operation was decided upon, and his colleague Mr. Cooper Forster was willing to undertake it. He commenced the operation by making an incision three and a half inches long in the course of the linea semilunaris on the left side, commencing at the cartilage below the eighth and ninth rib. On reaching the stomach in the left hypochondrium he seized it with a hook and fixed it to the sides of the incision. He then opened the organ and stitched the edges all round to the wound. Some warm water was then introduced by means of a tube, but the patient soon afterwards coughed, when most of it regurgitated. He had taken no anæsthetic for fear of vomiting, and the patient declared that he had no need of it as he was quite fearless of pain. A small quantity of milk and water was occasionally put into the stomach, but the man never seemed to rally, and he died forty-four hours after the operation, apparently from exhaustion. The post-mortem examination showed no peritonitis nor any special cause for his death. It was supposed to be due to shock in a man so extremely wasted from evident starvation. The cancerous tumour was quite localised, and growing at the back of the lower part of the larynx.

I have a few short articles on "Chronic Rheumatic Arthritis," "The Relative Importance of Aortic and Mitral Disease of the Heart," and also on "Contre-coup." This expression, I say, was intended to show

how a blow on one side of the head might injure the brain on the other side; but I found that many students, and even the senior ones who were dressers, were under the impression that a violent blow on one side of the skull might produce a fracture on the other side. This caused much hesitation on the part of the dresser as to which side he should operate, although the seat of the injury was evident by the scalp wound. I then had to explain that the most successful operations were those where the middle meningeal artery was injured by the fracture of the skull, and the trephine, therefore, allowed the surgeon to remove blood or a clot from the outside of the dura mater. For in this case an operation was not of much value where the dura mater was incised to reach an effusion of blood on the surface of the brain, seeing that in this case the brain itself was generally very much injured. If, then, the skull was very rarely fractured by contre-coup, no successful operation could in most cases be done on the opposite side to the injury.

Also, I have a short paper on "Acute and Chronic Disease." The remarks I make upon this subject would be considered indisputable at the present time, but when I first began my studentship this was by no means the case. It was generally taught, both in books and in oral lectures, that inflammation of an acute form might seize upon any organ, and this, if not cured, would become chronic; and, on the other hand, if a patient was suffering from chronic disease, or this was found on post-mortem examination, it was assumed that the patient

at some previous time had suffered from an acute affection. My paper was written to refute this opinion and show that there is no evidence from the history of most cases of chronic disease that it had been preceded by an acute inflammation, as, for example, there was no evidence in cirrhosis of the liver that there had been a previous hepatitis, or in a long-standing case of Bright's disease that there had been an antecedent nephritis. The changes both in the liver and in the kidney began as chronic diseases, if that term might be used, from the very commencement. The processes going on in their tissues were the same from the beginning, and then proceeding onwards until the organs were destroyed, or, at least, were no longer able to perform their full functions.

Mr. Cock relates "A Case of Pharyngotomy for the Extraction of a Foreign Body." It was the case of a young man who swallowed some false teeth, which stuck in the œsophagus. Being unable to extract them with forceps, Mr. Cock opened the œsophagus at the side of the neck, and so removed them. He states that this operation had only been done once or twice before.

Then there is a further paper by Dr. Pavy on "The Alleged Sugar-forming Function of the Liver," and on "The Influence of Diet on the Liver."

Then follows a "Case of Poisoning by Nicotine" by Dr. Alfred Taylor. A gentleman, aged 36 years, well acquainted with chemistry, took this alkaloid with the intention of committing suicide. He was

heard to fall, and on going to his room he was found lying on the floor and he soon after died. A small bottle of a brown liquid was at his side, and this, on being analysed, was found to be a solution of nicotine. Dr. Taylor gives an account of the chemical analysis and the appearance of the organs, especially how the blood had become altered in appearance. He adds that, as far as he knew, this is the only case of poisoning by nicotine which has occurred in this country. There is only one other on record; this occurred in Belgium, and was the subject of a trial for murder in 1851. The Count and Countess of Boarmé were charged with the murder of the Countess's brother by administering to him nicotine while dining with them, and he did not survive more than five minutes afterwards. The poison was found in the possession of the Count and he was executed. Then follows a long account of the chemical analysis by Dr. Hofmann.

Dr. Gull writes a very interesting and important paper on "Paraplegia." Amongst other cases he gives that of a man, aged 28 years, admitted under his care in 1857. Besides the paraplegic symptoms he had suffered all his life from some abdominal disease, shown by attacks of constipation and sickness which made him thin and feeble. These intestinal attacks still continued, and in one of them he died after having been three months in the hospital. The post-mortem examination showed a congenital misplacement of the cæcum and colon, but the immediate cause of death was peritonitis from a perforation caused by this. The symptoms con-

nected with his spinal system were the most interesting, as well as the appearance of the spinal cord, especially on microscopic examination. Although Dr. Gull gives no name to the disease it is clearly one of locomotor ataxy, as the drawing of the spinal cord, which accompanies the paper, well shows, the degeneration of the posterior columns being most marked. The patient stated that about eighteen months before admission he found his legs getting weak, so that he could not stand without support. In a recumbent posture he could flex and extend his legs with freedom, but if sitting up or standing his limbs moved with sudden jerks, as if he had not complete control over his muscles; he was also awkward in handling objects, as he was unable to grasp them; there was also some numbness of the hands and feet, and he did not expand his chest freely. He had never had syphilis. After having been carefully watched and his symptoms noted he died of peritonitis arising from congenital disease of the intestine, as just mentioned.

The spinal cord, which presented no altered appearance externally, was then hardened in spirit. Sections of it were afterwards made, and it was found that the posterior columns were diseased throughout their whole length. A complete degeneration had apparently been taking place, as they were changed into fatty and granular matter mixed with exudation-cells. The lateral columns appeared normal, as well as the roots of the nerves.

Dr. Gull goes on to state that these symptoms, together with the peculiar disease in the spinal cord,

tend to show the correctness of Dr. Todd's theory, when he says that the "posterior columns propagate the influence of that part of the encephalon which combines with the nerves of volition to regulate the locomotor forces, and to serve as commissures in harmonising the action of the several segments of the cord." In this case, Dr. Gull says, the want of power to regulate the action of the muscles was very characteristic. The legs, when drawn up, as they could be freely, were drawn up with a sudden jerk, and extended in the same manner. The voluntary movements were also fumbling and vague. This is an abbreviated account of the case, but in all particulars resembles what was known afterwards as locomotor ataxy and posterior sclerosis of the spinal cord.

The date of this paper by Gull is 1857, and it is the first description I have ever read of this form of disease; and as regards the drawings and illustrations I feel pretty confident that none had ever appeared like them, and especially for this reason, that the microscope had scarcely come into use for examination of the nerve structures. I do not myself remember any description of this form of disease until it was recognised in France as something new, and the name "locomotor ataxy" given to it by either Vulpian or Charcot.

Vol. 5, 1859.—In this volume Mr. Cooper Forster gives another case of "Gastrotomy," which he performed on a child for obstruction in the œsophagus.

The patient was a little boy just over four years of age. He was in an extremely emaciated condition. It appeared that seventeen weeks before he had swallowed some corrosive poison, supposed to be a solution of potash or caustic alkali, used for cleaning and bleaching linen. After a time difficulty of swallowing came on, and at last no food whatever could be taken. Under these circumstances the boy was brought to the hospital, and Mr. Forster, to whose care he was entrusted, seeing that the child was at the point of death from starvation, proposed to open the stomach. He performed this in the same manner as in his previous operation for a similar cause. The child was fed through the opening made in the left hypochondriac region, but he never thoroughly rallied, and died four days after the operation. There was found sloughing of the wound and peritonitis from giving way of the sutures. The œsophagus was narrowed, owing to a great thickening of the submucous tissue.

I have then a paper on Addison's disease, and I might say an important paper, for having assisted Addison in the publication of his work by collecting the cases and describing them, he requested me to undertake the examination of all other cases which he saw or were sent to him. As it might be supposed, specimens came in a considerable number, and at the end of two years they perfectly established the truth of his discovery. I relate in this paper one very marked case, as Addison considered it typical of the disease. The man was excessively dark in colour and *uniformly* so. On this point Addison

laid great emphasis, because cases had been often sent to him of discoloration in patches (in fact cases of leucoderma); it was true that he had figured in his book a case of this kind, but the patient did not suffer from the characteristic symptoms, nor was he very ill; neither was there any post-mortem examination made to corroborate the suspicion. So, after a two years' further experience, Addison entirely discarded the idea of there being any mottling or brown patches in supra-renal disease, or that any accidental disease like cancer might be the foundation of it. He had now entirely made up his mind that the destruction of the supra-renal organs was of a primary and peculiar character, and that this and the symptoms were more uniform than he had at first supposed. The four cases which were published in his book, and in which post-mortem examinations were made, constituted really the essential part of the work, and were most excellent examples of the true and characteristic *melasma suprarenale*.

This was the name originally given to the disease, as well as that of "bronzed skin." The term "Addison's disease" was given by Trousseau, the celebrated French physician, and the first man of distinction who accepted it as a newly described disease. The professor, after speaking of the propriety of calling diseases after the names of the discoverers, goes on to say: "C'est encore pour obéir à ce sentiment d'équité que je vous propose aujourd'hui d'imposer a la maladie dont un individu couché au No. 5 de notre Salle Sainte-Agnes nous a offert un

remarquable exemple, le nom du médecin anglais qui l'a decouverte. Ce médecin est le docteur Addison, le collaborateur de Bright, le doyen des professeurs du Guy's Hospital à Londres et depuis longtemps connu parmi nous par les travaux dont il a enriché la sciènce. Je propose donc d'appeler Maladie d'Addison cette singulière cachexie spécialement characterisée par un discoloration ou plutot par un coloration particulière, 'par la teinte bronzée' que prennent les téguments et qui a valu á la maladie le denomination 'de bronze disease' sous laquelle le docteur Addison l'a designée."

After this further experience of about two years since the publication of his work Addison had quite made up his mind as to the distinctive and primary nature of the disease; he saw that this long train of symptoms could not be owing merely to a destruction of the organs by a quick-growing adventitious tumour, but that it was really a special disease of these small organs. It stood on the same footing as a cirrhosed liver or granular kidney.

I might mention that before the publication of his work on 'Disease of the Supra-renal Capsules,' he had brought the subject before a local society called the "South London Medical Society," and his paper is to be found in the 'Medical Gazette' of March 15th, 1849. His subject was "Idiopathic Anæmia" and "Disease of the Supra-Renal Capsules." After giving a clinical description of the cases, the author said: "In three cases only was there an inspection of the body after death, and in *all of them was found a diseased condition of the*

supra-renal capsules" (the italics are in the original). "In two of the cases no other disease could be found in any other part of the body." Dr. Addison inquired "whether it could be possible for all this to be merely accidental." He thought not, and making every allowance for the bias and prejudice inseparable from the hope or vanity of an original discovery, he confessed he felt it very difficult to be persuaded that it was so. On the contrary, he could not help entertaining a very strong impression that these hitherto mysterious bodies—the supra-renal bodies—may be directly concerned in sanguinification; and that a diseased condition, structural or functional, may interfere with the elaboration of the body generally, or of the red particles more especially. At all events, he considered the time had arrived when he felt himself warranted in directing the attention of the profession to these curious facts." These remarks show a very natural and judicious form of prophecy, but which has taken half a century to prove to be the truth.

I take up the subject in this volume of "Melanæmia," first described by the French as a pigmented change or deposit which takes place in persons who have for a long time been exposed to the ague poison or miasmatic affection, as it was called at that time.* French authors mentioned it as more especially affecting the liver, and spoke of it as the "melanæmic liver. In my paper I more especially allude to two cases described by Bright in his well-known work, 'Report of Medical Cases.' He has

* Now known to be due to the bite of a gnat.

two beautiful drawings of the brains of a man and his wife which are completely black, due to the cause above named.

An old man and woman came into the hospital in the year 1829; they had come from Horncastle, in Lincolnshire, an excessively aguish locality, and walked the greater part of the way. They arrived in the middle of the night utterly exhausted, and a few days afterwards they both died. On post-mortem examination the brain, after removal of the dura mater, was seen to be completely black, and the same colour penetrated through the whole substance of the cineritious matter, and as Bright describes it, was seen to be in four layers. These no doubt were the ordinary layers of the cineritious substance rendered more distinct by the presence of unequal amounts of pigment.

I describe in this volume some wax models recently made, representing some peculiar forms of skin disease, and amongst others models of hands taken from students, showing inflammation at the roots of the nails, due to arsenic. At that time arsenic in solution was being used to inject the "subjects" in the dissecting room. After a few days many of the students found the ends of the fingers hot and tender, and at last very painful, when generally suppuration took place. At this time the nail became of a yellowish colour, and all round the nail was a bright pink line, and at the root a little pus exuding. These conditions are well shown in the models.

Dr. Gull gives a report of cases of cerebral

aneurysm. Also a case of pneumonia arising from pressure on the pneumogastric nerve by an aortic aneurysm.

Vol. 6, 1860.—In this volume I have a paper on "Some Diseases of Children." Under "Brain Affections" I notice the varying state of the pupils in meningitis and hydrocephalus, which I attributed directly to a local cause, as it seemed dependent upon the effusion acting on the optic nerve. I have already alluded to the fact of the pupils often varying in size, and that they alter according to the position or side on which the child lies. I also mention the apparent weakness or modified hemiplegia towards the end of the disease, which must denote some difference in the condition of the ganglia on the two sides, although this also might be attributable to the pressure of the increased ventricular fluid. I once had the opportunity of observing similar facts as regards the pupil in a man who had a large ventricular effusion. On my first visit to him I noticed that one pupil was larger than the other, and on my next visit I observed the same fact, but they were different eyes. I inquired of the nurse whether he was not lying on the opposite side on my first visit; she assured me that this was the case. I then directed her that on my next visit he should be lying on the side I first saw him; this was done, and again the lower pupil was enlarged, as on the first occasion. I could therefore come to no other conclusion than to suppose the gravity or

pressure of the fluid was enough to disturb the function of the thalamus, or some other part where the optic nerves had their origin.

I allude also to my teaching to students who sometimes used the terms "meningitis" and "arachnitis" as convertible, showing them how in the cases we call idiopathic the effusion was amongst the vessels of the pia mater, and beneath the cerebral arachnoid, whereas in the true arachnitis the effusion was between the two serous surfaces, and generally had its origin in the dural arachnoid, which again was set up by injury to the skull—a very important difference to be noted. I spoke of the similar conditions as regards the pleura. We might find a pleurisy accompanying a pneumonia; in that case a thin layer of lymph might appear on the surface of the inflamed lung, but in the true pleurisy which began as such, the effused fluid was between the costal and the visceral pleura, or within the cavity of the chest.

I speak then of "Chronic Hydrocephalus" in reference to a class of cases met with in adults where the head is large, but the subject of it is not ill, whilst at the same time he is weak in intellect and often gives much trouble to those who have charge of him or to his family if he lives at home. In such cases there may be some history of illness in infancy but never so severe as met with in the ordinary meningitis of children. It is not a case of acute meningitis becoming chronic as the terms "acute hydrocephalus" and "chronic hydrocephalus" would imply. At the same time there are indications of an inflammatory condition having occurred

at the base of the brain some time previously. This was Mr Hilton's belief, of which I first became aware when he asked me to make a post-mortem examination of one of his patients. The man had a large quantity of fluid in the lateral ventricles, and at the same time there was thickening and opacity of the arachnoid at the base of the brain, which, as Mr. Hilton pointed out, had completely closed the opening of the fourth ventricle below the cerebellum and so prevented the fluid passing down the sub-arachnoid space into the spine.

Subsequently a case of a similar kind came under my own notice. It was that of a gentleman, aged 50 years, who since childhood had been weak in intellect and had a very large head, so that he had always been obliged to live with some of his family in order that they might take care of him. At the time of his death he was living with his brother, who also had charge of his property. His sudden death caused a rumour in the small town in which he lived, and propagated by certain loquacious and slanderous persons, that there had been some foul play on the part of his brother. So an inquest was held and a post-mortem examination made, when there was found an enormous amount of fluid in the ventricles, which no doubt had been there all his life, and due to the thickening and adhesions of the arachnoid which were seen at the base of the brain.

A notorious case occurred a few years ago where a schoolmaster was charged with killing a boy as he died soon after a flogging. The boy had a very

large head and was very dull and stupid. An inquest was held, and at the post-mortem examination there was found a very large quantity of fluid in the ventricles, which no doubt was due to some localised inflammation in infancy. I forget whether the master received any punishment, but certainly he deserved it if he thought that he could thrust knowledge into a boy by thrashing him. I fear many disgraceful acts of this kind must have occurred in the old times of flogging. I remember at my own school, where corporal punishment was practised, that a very stupid and idiotic boy was repeatedly chastised by the master.

This volume contains a very interesting and well-known paper on the "Physiology of Sleep," by Arthur Durham. The general facts about the circulation varying during sleeping and waking were tolerably well known, and as these tallied with certain theoretical considerations, so nothing more was required than to verify the correctness of these by experimental methods. This Mr. Durham has done. He showed very clearly that during sleep the brain contains a minimum of blood, but in its active state the circulation is vigorous. It is remarkable, however, that he did not quote several authorities who had long maintained this principle, and Sir Astley Cooper had even demonstrated it on a patient. I will, however, first state what Dr. Macnish says in his work, 'The Philosophy of Sleep': "The greater the quantity of blood sent to an organ, the greater is the energy of its manifestations. Why should the brain be an exception to

the general law? So far from there being any increase of blood in the brain during healthy sleep, it is proved that the circulatory fluid in that organ is absolutely lessened, as I have had occasion to show in a case related by Blumenbach, of a person who had been trepanned and whose brain was observed to *sink* when he was asleep and swell out when he was awake. The abolition of the cerebral functions is to my mind sufficient evidence of diminished action going on in the brain. I cannot conceive increased circulation without augmented functional energy. When the brain is at work in the active state it is the most highly vascularised."

Dr. Lyon Playfair some years ago gave a lecture on the same subject, which may be regarded as the chemical side of the case. He showed that everything which diminished the supply of oxygen to the brain produced sleep, as it was often observed to occur after hæmorrhages, and a very usual time for somnolency was after dinner, when there was a diminished flow to the brain, as it was then especially directed to the stomach. The winter sleep of hybernating animals, he said, might be attributed to a deficient oxygenation of the blood, as cold contracts the blood-vessels and causes the circulation to be more sluggish. Sir Astley Cooper's case was that of a girl who had lost a large part of the calvaria from venereal disease, which laid bare a portion of the brain. When she was in a dreamless state her brain was motionless and lay within the cranium; when her sleep was imperfect and agitated by dreams her brain moved and protruded towards the opening;

when she was perfectly awake and engaged in conversation the protrusion was still greater. All this was brought to the proof by experiment, and the following is the account which Mr. Durham gives. He trephined a dog's skull and cut away sufficient dura mater to expose the brain. The portion of brain exposed appeared to rise through the opening in the bone; the large veins on the surface were distended and also the vessels of the pia mater. After the effects of the chloroform had passed off the dog sank apparently into a natural and healthy sleep. Corresponding changes then took place in the appearance of the brain: its surface became pale and sank below the level of the bone; the veins were no longer distended, a few small vessels containing blood of an arterial hue could be seen, and many vessels, which before were full of blood, were now scarcely observable. When the animal was roused a blush broke forth all over the brain, and when he was excited the brain became more and more turgid with blood and the surface a bright red colour. After a time the dog was fed, and then he sank into repose. The blood-vessels then resumed their normal appearance, and the surface of the brain became as before. The animal slept in a perfectly natural manner. The contrast between the appearances of the brain during the period of functional activity and during its state of repose or sleep was most remarkable. In order that he should in no way be mistaken, Mr. Durham operated on another dog and placed them side by side; the appearances during sleeping and waking were exactly the same.

When placed alternately in different states the comparison between the two was most striking.

Mr. Durham also has a paper on "Hermaphroditism." It consists of cases of deformed sexual organs, and in all those of doubtful sex there were found, either when living or after death, undeveloped or infantile testicles, but they all had more or less markedly the female form. Soon after this Mr. Durham had an opportunity of seeing a good example of this—not the patient during life, but the organs when removed after her death, which I showed him in the museum from a case of Dr. Oldham.

Then I go on to say that Dr. Oldham had a private patient, a lady in outward appearance, who came to consult him for amenorrhœa, or rather because she had never menstruated. On examination he found a complete absence of the uterus, but in each groin was a hard body, the nature of which he did not at first determine. She also told him that every month she felt these lumps somewhat painful and swollen. The doctor does not say what kind of question he put to the patient to elicit this answer, but it was enough for him to request her to come every month to be examined. After several visits he was quite convinced that they did enlarge as she had stated, and that they were really the ovaries misplaced. He considered he had made a great discovery by proving that these organs determined the phenomena of the periodic catamenia, and lost no time in sending the case to the Royal Society.

The case in this way got into the medical journals,

and was quoted in a students' text-book which shortly afterwards appeared (that of Tyler Smith). The patient subsequently fell into a consumption and two years afterwards she died. Dr. Oldham having carefully watched her, obtained consent to perform a post-mortem examination. He took with him the porter at Guy's, and on removal of the sexual organs with these two bodies, he told the porter to bring them to me at the museum as I was then Curator. I examined and found them to be testes, though small. It happened that Mr. Durham was then in the hospital and I requested that he should come and see me in the museum. I showed him these bodies and he did as I had just done myself; after making a section of them, he seized a portion with the forceps and pulled out long threads of "tubuli seminiferi." Dr. Oldham wished them to be returned to him so they were sent back to his house.

Dr. Oldham lived many years after this and died in the country at a great age. Not being able to find out anything about the preparation I went to the Royal Society and found in their records the case described as Dr. Oldham had stated (it was not published in the 'Philosophical Transactions'). I then made a note as to my discovery that the organs were testes and not ovaries, as had been thought by Dr. Oldham when he sent the case to the Royal Society two years before the patient's death. This note was placed in the book containing the original account.

It appears to me that this was a most marked

example of what usually occurs in these so-called "hermaphrodites"; that the great majority of cases are those of undeveloped males who have put on the feminine form, the rule being that the testes have not descended to be visible or felt externally. The same occurs, as far as I have had any experience, in the lower animals; for example, when the butcher has thought that the sheep which he has killed is a ewe he has found a testis within the body. I have been lately reading that permission was given by the Royal College of Surgeons for the preparations of John Hunter to be examined containing the malformed sexual organs of oxen, and which Hunter called "freemartins," and in all these non-descended testes were found.

I have elsewhere ('Lancet') contended that the so-called hermaphrodites are, as a rule, imperfect males. I have not yet seen or heard of any other kind. For the reason why there are not cases of imperfect females, I have thought that as the testis with its perfect function is naturally outside the body, when not developed it is in consequence of its not having descended from the place of its growth inside the body, but no accident of this kind could occur with the ovary, which is developed altogether and entirely within the body. I have always been of opinion that the term "hermaphrodite" is a misnomer; it seems impossible that the same ovum can develop two sexes at the same time, and that the outer appearances should partake of the characters of both. In some of the mollusca, which are bisexual, the male and female organs are developed from distinct structures.

The story of Hermes and Aphrodite being made into one person by the Greeks is a very pretty poem, but there the matter ends.

In previous numbers Mr. Birkett has discussed the various kinds of tumour which he has removed. In this volume he speaks more especially of cystic growth in the neck. These, he states, are generally congenital, an observation made by others, and especially by Sir Thomas Smith, who wrote an article on the subject. Since this time they have been shown, and especially by Mr. Bland-Sutton, to be remnants of the fœtal branchial arches.

Mr. Salter describes the anatomy and contents of an "Ovarian Dermoid Cyst." In this cyst there was the usual fatty matter secreted by the sebaceous follicles on its interior, and also a quantity of hair. The three teeth which were present were carefully examined by Mr. Salter. The most remarkable circumstance was the discovery of nerves in the tooth-pulp, the dentine having often been observed before. He mentions that Mr. Henry Gray gave an account of a tumour which contained some nerve-matter, and which he called a ganglion, or little brain, and in this he discovered a number of nerve-tubules.

Vol. 7, 1861.—This volume contains a further contribution to the subject of "Kiestine," by Dr. Braxton Hicks. The former one was by Dr. Golding Bird, in the year 1840, in vol. v. Series I, and was of a chemical character. In this paper he showed that

kiestine was of a fatty nature, and occurring in the urine of pregnant women he inferred that milk was being secreted in the mammæ, and then, passing into the blood, might be a source of nourishment to the developing fœtus. Dr. Hicks reports several cases, showing that the substance not only occurred in pregnant women, but that he had found it in virgins. In such cases it might be that there were other causes for excitation of the mammæ.

Then follows a long and elaborate paper of my own on the subject of "Pyæmia." It gives short reports of all the post-mortem examinations of cases which had occurred in the hospital for the previous eight years, and which amounted to about 150. I classify them according to the nature of the injury, from which it will be seen that in about half of them the bone was involved, and quite irrespective of the nature of the injury which caused it. In cases of compound fracture of the thigh it was quite the exception for recovery to take place; but even in lesser injuries to the fingers, if the bone was involved the hazard of blood-poisoning was still the same. It seemed as if the cancellous tissue must be involved to cause the disease, for it did not often occur after ordinary cuts or wounds, and was seldom observed after such operations as herniotomy. These cases of evident blood-poisoning were called "phlebitis," on the supposition that the veins took up the morbid material and carried it into the various parts of the body, but there being little proof of this, the term "pyæmia" was generally used. I myself believed that, the bone being so often involved, these cases

were due to the ready absorption of deleterious fluids through the open Haversian canals. It was very evident that there was also an external favouring cause for its occurrence, for a very strong belief prevailed that pyæmia was a disease of hospitals, and also that it was contagious, as several cases might be seen in one ward and none in another. If the disease increased beyond a certain degree the ward was closed and was thoroughly cleansed and fumigated.

I have more especially alluded to this paper written in the year 1861, because I had the opportunity of observing the remarkable result of Lister's new antiseptic treatment: instead of these cases of pyæmia coming every few days before me in the post-mortem from the surgical wards, they very rapidly ceased to appear, and then altogether stopped. Nothing could be more striking than this result of the new methods of treatment. I might add that such living elements as "bacteria" were altogether unknown. Nor did I hear any more of the alarm of the surgeon, who knew it was the patient's death-warrant when a few days following an operation he had a severe attack of shivering or rigors.*

In this volume Dr. Pavy makes further contributions to the subject of "Diabetes," and the influence of food on the disease.

Sir William Gull has an interesting paper on

* So recent has been the discovery of bacteria that on one occasion a member of the Pathological Society having mentioned this word, another member, a well-known physician, asked him what connection the disease had with frogs (batrachia).

"Paralysis of the Lower Extremities Consequent upon Disease of the Bladder and Kidneys (Urinary Paraplegia)." This was a form of disease described by Brown-Sequard, who, being a great authority on nervous diseases, was constantly being quoted by the profession. He described it as a paralysis of the legs, which followed, and was induced by, disease of the bladder, or, in his own words, a reflex paralysis in distinction to one of central origin. His meaning was that the bladder induced a reflex contraction of the blood-vessels of the spinal cord and so affected its nutrition and rendered it for a time altogether functionless. Gull, after discarding the doubtful cases, took some fatal cases, and in these he said the disease of the urinary organs was obvious, and when the cord was said to be healthy Brown-Sequard judged merely from the external appearance, he having made no microscopic examination; so also with the cases he quoted from Stanley's work published many years before the microscope had got into use. Gull then proceeds to say that on many occasions a cord which had appeared healthy was, on microscopic examination, found to have undergone most extensive degeneration. I might remark that, as far as I remember, Dr. Gull was the first to show the particular parts of the cord which were affected in different diseases, notably that of locomotor ataxy, although Waller had already described the mode in which degeneration of the nerve-centres took place.

I describe in this volume some new models of skin diseases. Some represented a case of glanders.

It shows lumps or swellings all over the body, some soft and others suppurating; between these is an eruption on the skin. This consists of small hard nodules, some of them suppurating and discharging dead cellular tissue; some have a close resemblance to the pustules of smallpox.

Another interesting model is what I have called "linear atrophy of the skin," and which I believe had never before been described. They resemble what is commonly seen on the abdomen of women who have had several children, or on the skin of the abdomen if it had previously been distended by ascitic fluid or ovarian tumour. The former have had the name of "linea gravidarum." But they seem to be formed under other conditions, and sometimes in young persons where there has been no evidence of any previous stretching of the skin. The first case I give was that of a girl, aged 18 years, and well grown. She was covered in various parts of the body and limbs with white lines or seams resembling the marks on the abdomen of a child-bearing woman. She stated that she had observed them coming about five years before as faint spots, and then gradually increased in size, and also that fresh spots were continually appearing. The largest was about four inches long and half an inch broad, stretching over the left hip, the ends tapering to points. The colour was a dead white as contrasted with the adjacent pink skin, slightly corrugated by transverse wrinkles, and depressed or raised according to circumstances. Thus if the limb be stretched the surface of the patch becomes

slightly lowered, whilst if the leg be contracted it swells up and becomes raised above the surrounding level. To the touch it feels soft, as if a piece of the skin had been removed or destroyed and the breach filled up with some soft material or only covered by cuticle. In fact the true skin has been lost, and its place as if taken by the coarser bands of areolar tissue which hold the sides together. The smaller marks run transversely round the legs and arms, and these smaller ones suggest scars from incised wounds. All these white marks are very tender and sensitive, so that the patient shrinks when they are touched.

Another case was that of a lad, aged 19 years, and of this there is a very excellent drawing. He had had a chronic disease of the right knee-joint for six years; on this there were two or three faint lines, but on the left one there was a marked white patch crossing the patella and a smaller one below it. All these and others were of the same nature as in the girl's case, and those on the leg and foot closely resembled scars resulting from a wound. The cause of this state of skin is by no means clear. Probably it has a somewhat kindred origin to diseases like keloid, morphœa, etc., which arise primarily without any known cause.

Vol. 8, 1862.—In this volume I published a long Report on "Disease of the Supra-renal Capsules or Morbus Addisonii." This paper contains all the cases which had come before me since the last report in 1859.

As I was still curator of the museum cases were continually coming into my hands for examination, for I need scarcely say that although many accepted Addison's statement as to his having discovered a malady not hitherto suspected, there still remained a large amount of scepticism as to its existence. Its great opponent at that time was Dr. Harley, who carried on the war for a considerable period. It was in consequence of this scepticism that I published this report, and was able to add twenty-five cases to the four which Addison had published in his original work. In all of these the symptoms were well marked and the change in the capsules equally unique or uniform. In only one case was the discoloration of the skin absent, and this was accounted for by the shortness of the illness and confirmed by the post-mortem examination. Both suprarenal bodies were enlarged and occupied by an albuminous matter with no sign of degeneration. This report proved that the disease was a more distinct and special one than Addison had first conceived. The symptoms were few, but well marked and uniform, and the disease in the organs themselves as characteristic (as I said in a former volume) as a cirrhosis of the liver or the granular atrophied kidney of Bright's disease. The different amount of discoloration Addison attributed to the stage of the complaint: in the early stages there would be less discoloration, and in the very chronic ones the dark colour would be extreme.

The patients' different lengths of illness depended probably upon other conditions, such as perfect rest,

or the obligation of continued work, until the time of complete inability to exertion arrived. It was this extreme nervous prostration or asthenia which Addison regarded as the most important symptom in supra-renal disease. He was not the man to theorise much, but he thought it most probable that the organic system of nerves were involved owing to the proximity of the cœliac axis and semi-lunar ganglia to these bodies. When at one of the medical societies his usual opponent placed many objections before him in disproof of its being a special disease, his answer was the following: "If he saw a patient who presented this peculiar discoloration of the skin, he offered no explanation as to why, or how it came; but if he observed associated with that discoloration a certain train and combination of symptoms such as a pearly eye, a feeble pulse, a disposition to strongly marked asthenia, he said, 'Here is a case in which there is disorganisation of the supra-renal capsules; and if the body is examined you will find no other organs are diseased.'"

About this time Mr. Bader was appointed Ophthalmic Surgeon. He brought with him from Germany the new methods of examining the interior of the eye, having been a pupil of Von Gräfe, who used the new instrument invented by Helmholtz—the ophthalmoscope. He gives particulars of a patient who was admitted into the hospital on account of blindness and convulsions, together with a strangeness of manner. He was a man, aged 32 years, strong and muscular, in a stupid condition, like drunkenness, and continually trying to turn over on

his left side. A long account of the ophthalmoscopic appearances is given, and other symptoms, such as deafness, ensuing in addition to the impairment of sight. After death Mr. Bader made a section of the eyes, and gives particulars of the changes which had occurred in them. The cause of all the patient's troubles was found to be a tumour the size of a small hen's egg occupying the lower surface of the right lobe of the cerebellum, or, as Mr. Bader states, it seemed to grow from the membranes, and only pressed on the cerebellum, in which it formed a deep pit. The nature of the tumour is given, which at the present time would be called a glioma.

In this volume Dr. Gull publishes a case of progressive atrophy of the muscles of the arms in a tailor, aged 42 years. He caught typhus fever in the hospital and died (at that time this disease was admitted into the general wards). On examining the spine there was found a long cavity in the middle of the cord, beginning at the fifth cervical vertebra, and gradually enlarging as it passed downwards to the seventh cervical; it then became smaller, and ended at the third dorsal vertebra. The surface of the cavity was smooth, and it tapered at both ends, the large middle part being of a square form. Dr. Gull considered it to be an enlargement of the central canal, and called it a "chronic hydromyelitis," comparable to a chronic hydrocephalus.

Dr. Moxon describes the case of a subject which was in the dissecting-room in the winter session of 1860–61. The woman had died of phthisis. All the peripheral nerves were about three times

their natural size. He examined them carefully and microscopically with the aid of Professor Quekett, and they found that the increased size of the nerve did not depend upon an additional number of the ultimate fibrils, but that the latter themselves were enlarged; by exact measurement these varied from twice to thrice the natural size. From what I myself remember, the ulnar nerve was as big as the little finger, and the popliteal as large, or perhaps larger, than the middle finger.

In describing some new wax models, I mention one that was made at my suggestion, of the peculiar warty condition of the knuckles of the hands which occurs in those who are constantly making post-mortem examinations. I found that it was not usually recognised, and in some cases treated as syphilitic, and so thought that a wax model of this might be instructive. It is usually seen on the knuckles of the fore- and middle fingers of both hands; these patches seem to be composed of dry, brittle epithelium of a brown colour, and about the size of a shilling. There is probably some irritating morbid fluid which produces these rough patches, but there seems to be no fear of blood-poisoning by absorption, or even lymphatic inflammation. Owing to their warty appearance, I proposed the name "verruca necrogenica," which seems to be generally adopted.

Vol. 9, 1863.—The first paper in this volume is by myself, "On the Syphilitic Affections of Internal Organs." I regard this as the most noteworthy and

original article which it has been my good fortune to write, having obtained for me one of my highest honours. When I say my good fortune, I must explain that the regular study of morbid anatomy, which must precede our knowledge of a larger pathology, is of recent growth, and even when I entered as a student at Guy's Hospital had not reached the dignity of a subject for teaching. The post-mortem examinations were made for the purpose of diagnosis, and to ascertain the nature of the disease from which the patient died. A printed form had to be filled up by the surgeon or physician, stating that the case was one of interest, and an examination after death would be valuable. This was delivered to the steward, and permission was generally obtained. This method had just been cancelled when I first began my studentship, and the simpler plan of asking the medical attendant if he would like an examination was adopted. I well remember when a physician was told that one of his patients was dead, and the usual question asked as to the necroscopy, that he replied: "No, it was not necessary, as it was a clear case of phthisis." It soon after happened that a man in a surgical ward died after a very long illness due to syphilis. A large part of the cranium was carious, the scalp had partly sloughed away, and hair destroyed, producing a large sore cicatrix on the head. There were also some other bones in the body involved. In this case no post-mortem examination was wished for, as the cause of death was too evident; but on my part the wish was quite the contrary, and so the body

was opened. I need scarcely say that the interior of the body was a perfect revelation to me. Amongst other notable things was a gummatous mass in the liver, which I described as a fibrous material with some rays of the same substance around it. This I showed at the Pathological Society, and a drawing of it is in the 'Transactions.' This was the first specimen of the kind exhibited in London, and was received with considerable incredulity as to its nature by many of the members.

I naturally examined many foreign works, in order to find if anything was known on the subject of "internal syphilis," and I found that Wedl spoke of having read a work by Dietrich, in which he mentions that the liver may be affected with syphilis. I endeavoured to obtain this work, but failed. In this report I describe these fibrous masses or gummata in the different organs, and as regards the lungs, I believe my specimen of syphilis of the lung was the first of the kind exhibited at any medical society. These were rounded masses, consisting of material exactly like that in the liver, and some of these undergoing softening. As regards the brain, my specimen shows the dura mater adherent to the brain over the right hemisphere in front.

This case exactly corresponds with those which Bright relates in his large work, called 'Report of Medical Cases.' These I have already mentioned where Bright describes a peculiar form of epilepsy, in which only one side is affected by convulsive movements, and there is no loss of consciousness.

In this manner, he says, they differ from ordinary epilepsy, where the whole brain is affected, shown by the general convulsions and the total loss of consciousness. These cases were mostly syphilitic, unless the adhesion had been caused by an injury. I also described syphilitic disease of the arteries, and how this differed from the ordinary atheroma by there being formed hard nodules in the walls of the vessels, and so often reducing their calibre. Some time afterwards I am quoted by Sir Clifford Allbutt in his report of a case of cerebral disease in a syphilitic patient, which he published in the 'St. George's Hospital Reports' for 1868. I refer to syphilis affecting the aorta and larger vessels, not only by producing characteristic firm patches in the interior, but by producing aneurysm. I had a young woman—a prostitute—in the hospital who had syphilis and an aneurysm of the abdominal artery. I also knew intimately three gentlemen, all between thirty and forty, who died of aneurysm of the aorta. They all had had inveterate syphilis.

I have alluded to the incredulity which existed for a considerable time after I exhibited my specimens at the Pathological Society, and it required two or three years before it was generally admitted that the whole body might be affected by the syphilitic virus. It was thought that the external parts of the body were alone affected, and that the virus only showed manifestations of its presence in the original sore, the lymphatic glandular induration, ulcerated tongue and throat, or troubles at any of the ordinary openings of the body. It was denied

that the interior of the body could be in like manner affected. In the year 1868, however, this fact was generally accepted, as I have good reason to remember, for my friend and colleague Mr. Salter gave my name to the Royal Society as an eligible member for the fellowship. It was based on my pathological work, but originality or an addition to the present knowledge of any subject was a surer way into the portals of the Society. This I was able to show by a criticism of my published lectures on Pathological Anatomy, which appeared in the early part of the year 1859. Shortly afterwards they were noticed in a medical journal of the same year. I give the quotation: "In pathological science Dr. Wilks, as one would expect, is *au courant* with the most modern authorities, but we cannot but think that he errs in laying down as law what is considered by most authors as still *sub judice*. For example, at page 329 '*syphilitic fibroid deposit*' is mentioned of frequent occurrence and of unquestioned character. Now most of those who have paid attention to the subject regard the proof of the statement of Dr. Wilks as anything but convincing, and, indeed, of the four cases published in the last volume of the 'Transactions of the Pathological Society' in support of his views, only one was known to have syphilis."

This adverse criticism proved to be my best friend, showing my originality, and thus obtained for me the Fellowship of the Royal Society.

The value of this knowledge was of the most practical kind, for there was reason to suppose that

potassium iodide would not remove gummata of the interior organs as well as those on the exterior of the body. I had not long to wait before I witnessed many remarkable instances bearing on this. A patient had a large nodulous liver and was under the care of a well-known homœopathic doctor, who told the man's friends that he had cancer and could not be cured. I saw every reason to believe it was syphilitic, and so gave him a course of iodide. The liver soon became smaller and the patient got perfectly well. Not long afterwards a young man called upon me, who said he was in the Post Office at Cape Colony; that he had been discharged for an incurable disease of the liver, and that he was going back to his mother, who lived in the north of England. He was recommended to call upon me by his doctor, who had been a Guy's student, on his way through London. He had a large liver in which I could feel two hard lumps; he confessed to having had syphilis when I interrogated him. So I wrote him a prescription of potassium iodide and told him to take it regularly, and if he was better and saw no other medical man to continue it for a month; then he was to write or call upon me. At the expiration of this time he called upon me and told me cheerfully that he had taken the medicine regularly, and thought he was quite well; he intended also, if I allowed it, to return to the Cape and resume his work. On examination I found the enlargement of the liver had disappeared, and only with some difficulty could I feel any induration at the edge of the liver. He returned to the Cape, but I

never heard whether or not he was taken back by the Post Office. One reflection I made at the time was to the effect that, not wishing to take a fee on his second visit, I only asked him to call upon me; he had been cured of his so-called cancer and again set up in life for the sum of two guineas, showing that the pecuniary rewards in the profession have nothing to do with the doctor's skill.

I must mention one more case, which made a great impression upon me in the early years of my practice, of the importance of making a correct diagnosis before prescribing medicines. The case was that of a young man who was ill enough to take to his bed, as he had continuous vomiting for several days; he had had an homœopath to see him, but giving him no relief after having tried every remedy in his pharmacopœia for sickness was superseded by the gentleman I met. The latter admitted that although he gave different medicines from his predecessor, he had endeavoured to overcome the continuous sickness by all the remedies and methods with which he was acquainted, but they were of no avail. I found the young man lying on his side, not caring to speak, but complaining bitterly of the pain on one side of his head; I then turned him on his back and observed one of his eyes inflamed, and on closer examination saw that he had iritis; so I proceeded to further examine him. I found some small indurated glands in the groin, there were a few spots of a doubtful rash and a large lump in the epididymis and a node on the tibia, so I made up my mind as to the nature of

the case. We then left the room to consult together, and the first thing I said was that I should recommend giving him iodide of potassium, whereat my friend said that he had never before heard of this drug being a remedy for sickness. I quite concurred, and I said I did not give it directly with that object, for I was in the habit of treating the disease and not the symptoms, and therefore as believing that this young man was suffering from syphilis I should recommend the iodide, and if that removed the irritating cause in the brain which was the source of the symptoms, then the sickness would speedily depart. So my friend assented and we prescribed the iodide. After a few doses the sickness was less, on the following day it ceased and the pain in the head was better, on the fifth day he was sitting up in his room and then rapidly recovered. He was soon able to go out, and at the end of the month he declared he was as well as he had ever been.

Then came a class of cases which was more important than all, those cases which Bright had described as not true epilepsy, where the patient struggled in every limb and became quite insensible. These exceptional cases were peculiar, showing, as he said, that the whole brain could not be affected because consciousness remained and the patient was convulsed on one side only. In these cases he had found on post-mortem examination that one side of the brain had been affected by inflammation and the membranes were adherent to the cerebral surface. Bright knew no more than others as to the cause of

this adherence, but now we have ascertained that the usual cause is syphilis we have the remedy in the potassium iodide. It is worthy of mention that in this paper on visceral syphilis I raise the question whether it affects the placenta as well as the child when the mother is contaminated, and so is the frequent cause of her abortions. The question arose in consequence of my predecessor, Mr. Wilkinson King, who died in the year 1847, having left behind him notes of several cases of abortion in which he supposed that syphilitic disease of the placenta was the cause. He gives a few cases of the kind, but they are not defined enough and too short to enable me to express an opinion about them, especially as the fibrinous effusion of which he speaks may have been merely the remains of extravasated blood. But he mentions one or two cases which probably are of the nature he suggests: one is that of a woman, formerly a nymph of the *pavé*, and who had aborted several times, when she again miscarried at the fourth month, and Mr. W. King examined the placenta and said, "The chorion was found thickened and coriaceous with adventitious membrane; funis distended. She was treated with blue pill and sarsaparilla. The patient has since been delivered of a female living child and is again pregnant." He mentions another case where he says the placenta was coriaceous. In a third where the placenta was adherent to the uterus by patches of a grey colour, the chorion was thickened by the same material of an eighth of an inch thick.

Vol. 10, 1864.—In this volume Dr. Braxton Hicks has an article on the "Nature of Proliferous Disease of the Ovary." He shows that this is distinctly an adenoid growth and has its origin in the ovum, in the same way as the adenoma of the breast grows from the glandular tissue. He speaks of Mr. Spencer Wells having described a similar case at the Pathological Society the year before, in 1863.

In a previous volume for 1856 I had a paper on lardaceous waxy or amyloid disease, showing more especially how frequently it was associated with syphilis and disease of the bones. In the present volume Dr. Pavy takes up the subject as to the nature of the substance known by these names, and discusses it both in its physical and chemical characters. The name "amyloid," I believe, was given to it by Virchow, as meaning a starch-like substance. There was an old idea that the function of the vegetable kingdom was to form or build up organic matter and the function of the animal kingdom to destroy it. This opinion might have influenced many persons in considering the nature of this material of which I am speaking; it was therefore a bold doctrine to assert that starch or some allied vegetable substance could be formed in the animal body. But the old idea seemed to be upset when starch was thought to be formed in the human body, although sugar, the source of which had always been known as existing in the vegetable kingdom, was now found to be formed in the animal economy. The idea of amyloid being starch was due to the fact,

as it was asserted, that it changed colour on the application of iodine. Dr. Pavy, on entering upon the subject of its chemical nature, stated that it was well known that pure vegetable starch turned a deep blue on the application of iodine; but in the case of cellulose and dextrine, iodine produced a colour of a reddish tint, and in some other substances a deeper red. In all these cases there seemed no chemical change produced but a mere staining or saturation of the tissue with the iodine. In this way Dr. Pavy regarded the coloration of lardaceous matter with iodine, and also from the fact that a chemical analysis showed it to contain nitrogen, which at once removed it from vegetable substances. Dr. Odling also made an analysis of a like kind with the same result. The term "amyloid," meaning starchy, is therefore perfectly incorrect. Iodine nevertheless is useful as a test, for lardaceous material being readily coloured by it, this new and adventitious substance is at once made evident in the post-mortem room. The term "degenerative" is also not the term which should be used for the lardaceous change; it is really a peculiar animal substance deposited from the blood, but the immediate cause of this has yet to be discovered.

In this volume is also a paper by Arthur Durham on mollities ossium. In this he mentions a change in bone, called *osteoporosis*, which seems to be the same as Paget's *osteitis deformans*. This is again discussed by me in a later volume.

Vol. 11, 1865.—In this volume I have another short paper on Addison's disease, in order to show that the profession, and especially that on the continent, was still sceptical as to the existence of a special disease of the supra-renal capsules, productive of the symptoms which the author described. Then I explain how this was in great part owing to Addison not perceiving in the first place that the disease now known by his name was even more specific than he had ever thought. For unfortunately he had placed in his work, besides the four cases of genuine examples of the disease, some others which were evidently not, Addison seeming impressed with the idea that any form of destruction of the adrenals would produce discoloration of the skin. He gave two drawings of patients with white and brown spots on the body, one case where the skin was much darkened in the region of the axilla, in the other where white and brown spots were scattered over it, and which was probably no more than leucoderma. This uncertainty in the original work, both as to the kind of discoloration and the nature of changes in the capsules, led to much sceptism, so that many doubted the reality of the newly discovered disease.

Thus, Professor Mattei gives an account of 310 subjects which he had examined, and amongst these he found two supra-renal capsules affected with apoplexy; there was also one of cancer, one of adipose tumour, four of tuberculosis, and others affected in several ways. In none did the symptoms

which Addison described exist. He states that he so often found effusion of blood in these organs that this condition may imply some congestion in the neighbouring vessels.

I don't think it very remarkable that in 310 cases he had not come across a real case of Addison's disease. About the same time M. Louis Martineau published a treatise, 'De la Maladie d'Addison." He adopted a similar method, and put together all the cases of disease of the supra-renal organs which he found post-mortem, and amongst these there seems to be one genuine case which he calls tubercular, but attaches no importance to it, for he says that a disease which may be cancerous or tubercular cannot have any definite character. In this respect the Professor speaks rightly; and he then further explains that the discoloration of the skin described by Addison is nothing more than the melanæmia spoken of by Jaccoud and Graves. His conclusion, therefore, is that the alteration of the capsules cannot be regarded as the only cause of the malady of Addison: "Il est evident que la maladie d'Addison ne pourrait être considerée comme entité pathologique."

Mattei also alludes to experiments on animals by destroying the capsules and how quickly death follows. This he repeated himself, and says: "In a rabbit I violently compressed both capsules at the same moment. The animal gave a sharp cry and fell into a state of prostration. Respiration almost ceased and in a few minutes it was dead." He continues by saying that he believes this was due to

the compression of the sympathetic ganglia. This was Addison's own theory to account for the excessive prostration and long-continued pulselessness of the patient.

There is no doubt that Addison's book gave a great stimulus to the question as to the function of the organ. Nagel, Frey and some others speak of the numerous nerves the capsules receive. Kölliker thinks that the two substances which compose these organs possess different functions. He thinks the cortical substance belongs to the vascular glands and has a function of secretion, but with regard to the great nervous richness of the medullary substance, he considers it an apparatus forming part of the nervous system. But my note of what Vulpian says is the most interesting of all in connection with the light thrown upon the function of these organs at the present day. He says that the blood issuing from the supra-renal vein has peculiar properties. It gives a characteristic chemical reaction, showing a peculiar substance which is formed in the structure of the organ and is poured into the blood. Sesquichloride of iron gives a bluish tint.

Vol. 12, 1866.—In this volume is a paper on "Hydrophobia" by Mr. Cooper Forster. He gives an account of the cases which had occurred in the hospital under the care of both physicians and surgeons for a long period of years. In nearly all these cases medicines had been given but had proved to be of no value. It is worthy of note to see how

the disease stands at the present time. I think I am right in saying that no case has occurred in this country for the last ten years, and for this happy state of things we are especially indebted to Mr. Long. When Home Secretary of State he clearly saw that rabies in the dog was due to a specific poison, and therefore when a dog was supposed to be mad it was ordered to be killed and the body to be sent to the Brown Institute for examination. If it was found to have the disease (which was ascertained by the experimental method) a cordon of forty miles was made round the place where the dog had been killed; at the same time no dog was to be admitted into the country without undergoing quarantine, or rather, a fortnight's seclusion. In this way this frightful disease was gradually, but most effectually, eradicated. All praise is due to Mr. Long in his perseverance in this truly scientific method, considering the opposition and even abuse which he received.

Then follows a paper by myself on "The Pathology of Nerve Diseases." I have refreshed my memory with great interest in reading this paper, for it is really remarkable to compare our present knowledge, small as it may be, with the crude ideas, or absolute ignorance, which existed at the time of my studentship of the anatomy and physiology of the cerebral structures. It was enough then to make a horizontal section of the brain and call it the *centrum ovale minus*, and then another section lower down on a level with the ventricles and call it the *centrum ovale majus*. Then the various parts

of the brain were named after their appearance, and no attempt was made to trace their connection with other parts so as to endeavour to ascertain their function. As regards the spinal cord even less seemed to be known, as the only term which was used when this organ was diseased or injured was *paraplegia*. It is comparatively recently, or since my student days, that the various forms of disease of the spinal cord have been ascertained and appropriate names given to them. It was known, this having been ascertained by Sir Charles Bell, that there were distinctly motor and sensory nerves; and afterwards by Marshall Hall that these came off from the tracts of the cord on its anterior and posterior surface, leaving the central part of the cord, which had a function of its own. The fact that a sudden affection of the brain might produce a paralysis of the opposite side was known to ancient writers. So in my time we had a knowledge of a lateral motor tract and also of a sensitive tract, these ending in the corpus striatum and optic thalamus respectively; the cerebral hemispheres with which mental processes were associated received the sensory impulses through the medullary matter between them and sent motor impulses through the medullary matter to the corpus striatum.

About this period my old friend, Dr. Hughlings Jackson, was interesting himself in the study of the nervous system, and sent me his various pamphlets, mostly from the 'London Hospital Reports' and the 'Ophthalmic Hospital Reports,' and I cannot therefore do better than state in his own words how

this theory stood at that time—that is, 1866, or in the three previous years: "The optic thalamus is supposed by some eminent physiologists to be the chief focus of the sensory nerves. The sensory tract may be traced upwards until it almost entirely spreads itself through the substance of the thalamus. It is supposed that the corpora striata stand in the same relation to the thalami optici as the anterior horns of the grey matter of the spinal cord do to the posterior horns, the corpus striatum being considered a motor and the thalamus opticus a sensory centre." I may say that difficulties had already arisen in accepting these doctrines as absolutely correct, in consequence of cases having arisen where a unilateral meningitis was shown after death and an apparent hemiplegia had appeared during life; but as these cases generally occurred after an injury, it was thought that the corpus striatum, or the parts around it, especially on that side, had received some shock or change. About this time Sir Jonathan Hutchinson, in publishing a series of cases of injury to the head (Astley Cooper Prize?), mentioned several where a certain amount of paralysis had occurred in connection with meningitis.

It was soon after this that some experiments were made on the brains of living animals by some German physiologists, whose names I forget, and followed by Professor Ferrier, showing that when the convolutions were excited by galvanic currents, movement of the limbs took place. Dr. Hughlings Jackson, who was working at the subject of nervous diseases, endeavoured to bring these

new facts to a practical issue, and found that while in ordinary epilepsy the whole body is convulsed and the patient unconscious, in other cases only one side is convulsed and the consciousness remains. Now, this was the form that Bright described in an early number of these 'Reports,' with his conclusion about it, in these words: "Had the convulsions occurred on one side only, and the patient remained conscious, he generally found some disease of one hemisphere and the dura mater adherent."

In these cases Jackson was able to distinguish the particular convolutions affected, by the special movements of the limbs and fingers. This affection, which is not true epilepsy, but acquired from the presence of disease, usually an adherent dura mater and of a syphilitic character, as I have already described, has been appropriately named "Jacksonian epilepsy," as Jackson had devoted so much time and attention to cerebral disease and its symptoms; with this name I fully accord as quite in harmony with the usual nomenclature of naming diseases after their first discoverer or interpreter.

As regards the experiments which led to this knowledge, I should say that Professor Ferrier was summoned to a police court for cruelty to animals in connection with the dog which had been experimented on, but as it belonged to the German Professor the proceedings against him were not continued. But in view of any future prosecution being established, the profession proposed to raise money to defend the defendants, whoever they might be. A large meeting of the profession took

place at the College of Physicians on the subject on June 7th, 1882, and at which many eminent men were present, including a well-known judge, and a proposition was made to invite all the Presidents and Council of the Royal Colleges of the United Kingdom to hold similar positions in the new association which was about to be formed. The name of this was then discussed, and Sir Wm. Gull said that we should not only raise money to defend any future person who might be prosecuted, but that we ourselves should make use of part of the money which we hoped to obtain in order to follow the experimental method which Ferrier had been doing, and he advised that the name should be "The Association for the Advancement of Medicine by Research." The subscriptions rapidly came in, beginning with Mr. Bowman, the oculist, and other distinguished men like Darwin, who gave £100. I have notes of all these particulars relating to the Society, and, therefore, that I was chosen treasurer and Mr. Stephen Paget secretary.

It was about this time it was observed that loss of speech accompanied only hemiplegia of the right side, and therefore the organ of speech seemed to be located on the left side of the brain only. This seemed very remarkable until my colleague, Dr. Moxon, rightly interpreted it. It had already been observed that the patient was not paralysed in the organs of speech, for he could make sounds as before; neither had he lost the knowledge of language, for he could read words and know their meaning, but he could not speak them.

Dr. Moxon explained that the left side of the brain, that is, the frontal part, was educated to use the right hand rather than the left, and in like manner the same part was educated to use the organs or muscles of speech. So, just as the loss of power in the right arm followed disease of the left corpus striatum, so in like manner loss of speech accompanied it. The proof existed in the fact, as authoritatively stated, that there had been cases of left-handed persons being struck with paralysis on the left side, when they not only lost the power of writing, but also the power of speech. This explanation for aphasia in right hemiplegia, which seems very simple, is that which is usually given. The aphasic patient may be again taught like a child, taking syllable by syllable.

I mention a case bearing on the subject, referring first of all to the cases published by Mr. Jonathan Hutchinson, when there was hemiplegia associated with an arachnitis of one hemisphere. My case was that of a woman who struck her head several months before her death, which produced a dulness of intellect, but she was sensible to all around her. She was also hemiplegic on the left side, but of this she was quite unaware, never seeming conscious that she had lost the power of using the limbs of that side. After death the whole surface of the right hemisphere was diseased, looking as if worm-eaten, all the layers of the cineritious substance being involved; the left hemisphere was quite healthy.

Then follows in this volume a case of "Excision of the Spleen," by Mr. Bryant. He was led to per-

form the operation by arguments in its favour, and which I myself supported. These were that the organ had been removed in the lower animals with impunity, and occasionally the spleen had been destroyed accidentally in man without any untoward result. From this point of view its removal seemed justifiable, although in the case mentioned the spleen was healthy, whilst in the other it was very much enlarged, and the blood showed an abnormal condition by the excess of white corpuscles. Nevertheless, the patient on whom it was thought desirable to attempt the removal gave his fullest consent, and so Mr. Bryant operated. The patient, a young man, had an enormously enlarged spleen, and this was adherent to the peritoneum in many places, so that on the attempt at removal these had to be torn away, when a large hæmorrhage followed, and he died in a few hours.

In the following year Mr. Bryant again attempted the operation in a woman, aged 40 years. She also died shortly after the operation for its removal. Soon afterwards Mr. Spencer Wells removed the spleen, but the case came to the same speedy ending. So far as these cases went the operation did not seem justifiable.

Then follows a further report on the "Treatment of Rheumatic Fever," by Dr. Rees and Dr. Sutton. They showed, as Dr. Gull has previously done, that the course of this disease, when no medicines were given, was as short as when various kinds of medicine had been administered in the usual manner. From this they drew the moral that every disease

should be studied so as to obtain its correct natural history, and then we should be in a position to satisfactorily ascertain if there was any drug which would at all influence its course.

This volume also contains another paper by Mr. Bader "On the Structure of the Retina and its Diseases." It is accompanied by some excellent illustrations, which are of the more value as they were done by the aid of the ophthalmoscope, which he was one of the first to use in England. Mr. Bader was, it may be remembered, a German, and had been a pupil of Von Gräfe.

Mr. Hinton also has a paper styled "On Some Morbid Conditions of the Membrana Tympani." I more particularly mention it on account of the beautiful coloured illustrations which accompany it, done, I believe, by his wife, Mrs. Hinton.

Vol. 13, 1867.—Dr. Fagge has a lengthy paper in this volume on some "Local Forms of Skin Diseases," as keloid, scleriasis, etc., illustrated by a great variety of cases.

Mr. Cooper Forster writes on a new method of "Arresting Hæmorrhage by Acupressure." This is done by passing needles under the vessel and then tying them by a thread on both sides of the wound. This was first done at Aberdeen, but I am under the impression that the method was attended by some hazard and was very soon discarded.

Dr. Moxon then gives a further list of cases, showing the importance of recognising "visceral syphilis."

Vol. 14, 1868.—In this volume is a long paper by myself, entitled, "On the Nature and Causes of Disease." It is a very comprehensive essay, and would have read better if it had been called the "Study of the Nature and Causes of Disease." It refers to the usual method of giving a disease a name, and to this very often the treatment is directed. To this I particularly call attention, as the question of heredity, temperament, habit and various surrounding circumstances ought also to be considered.

This is followed by a paper by Dr. Fagge on "Splenic Tumours," with illustrations.

Vol. 15, 1870.—This volume is interesting as containing a paper on the "Clinical Thermometer," contributed by the Editors, but compiled by Dr. Goodhart, the House-Physician. At that time the instrument had not come into general use, and was looked upon as a curiosity. It was of great length, and was shown at a medical society as the new medical discovery, but of very great importance. It became a subject of further discussion as to what part of the body should be chosen for its use, and the length of time it should remain there.

This volume contains a description of a case of "Exophthalmic Goitre," with a post-mortem examination, and a drawing of the patient when alive. This is the only directly fatal case I had seen, and which enabled me to make a post-mortem examination. The patient was a young man, aged 26 years, and it was only six months before when the

swelling was noticed. During that time the gland had grown to an enormous size, and the eyes appeared as if starting out of the head; indeed, he showed in the most marked manner an extreme instance of the disease. Already he was suffering from pressure of the tumour on the trachea, as he had considerable dyspepsia and bronchial trouble. With this was much secretion and expectoration, and he shortly died from the mechanical cause. The post-mortem examination showed nothing to cause death in any of the vital organs, all of them appearing healthy. The enlarged thyroid showed nothing in its appearance which differed from the healthy gland. The cervical ganglia and the sympathetic nerves were throughly examined by Dr. Moxon, but he discerned nothing abnormal in the cells and fibrils. There was, indeed, nothing to show that the cervical ganglia were in any way diseased. The blood was not examined.

There is a paper by Dr. Hilton Fagge on "Diseases of the Nails." In this communication he alludes to the marks on the nails occurring after any severe illness, and also alludes to my first description of it in the 'Lancet' for 1868.

There is a paper by myself, entitled "Arterial Pyæmia." I was speaking of the disease now known as "endocarditis and embolism." The name I gave seemed appropriate for those cases which I had observed where inflammatory products were carried from a cardiac valve along the arteries, so as to stop the circulation into the smaller ones in the viscera, and so block them. This affection I called

arterial pyæmia, because in a similar way it was generally allowed that some morbid material might be carried by the veins in a wound or injured part to various parts of the body, and especially to the lungs, producing special inflammatory processes, accompanied by feverish symptoms, and which we now call embolism. These, being marked by frequent rigors or shivering, were often called cases of ague. This was so in the case of a medical man in the south of London, who had some of the most eminent men in the profession to see him. A well-known surgeon was sure it was pyæmia, although he could not find the spot which gave rise to it. I made the post-mortem examination, and found old vegetations on the heart, with emboli in the spleen and kidney.

In a similar case of a lady, which was also seen by Dr. Gull, near Regent's Park, and called by him "ague," the bruit which was known to exist in both these cases was disregarded as having no relation to the disease. I gave a lecture on this subject, which was printed in the 'British Medical Journal' for March, 1868. The latter case is described by Mr. Adams (whose patient the lady was) in the 'Transactions of the Pathological Society.' This is the first case ever published. It is curious that although Dr. Kirkes, of St. Bartholomew's Hospital, had written a paper on the subject of vegetations of the heart, and how they might plug the arteries, yet the second part of his paper, referring to the smaller arteries and the parenchymatous organs, seemed quite overlooked (see 'Med.-Chir. Trans.' for 1852).

Vol. 16, 1871.—In this volume there are a good many papers of interest; one is by Dr. Fagge "On the Sounds of the Heart," dwelling more particularly on the newly established fact, or, as some said, the new discovery, of the existence of a presystolic bruit, caused by the blood passing through a narrowed mitral orifice from auricle to ventricle. It was therefore an auricular murmur coming before the contraction of the ventricle, and so giving rise to a presystolic murmur. Dr. Fagge showed that the committee of investigation formed soon after Laennec's time thought that a narrowed mitral orifice must produce a "bruit"; but it is not very clear whether they made any difference in time between this and the loud bruit which occurred when the blood regurgitated through the orifice; that is, whether, indeed, they distinguished the bruit caused by obstruction from that of regurgitation. As a matter of fact the senior physician to the Hospital did not distinguish between these two conditions, both bruits being called systolic. It should be remembered, however, that ten years before this time Dr. Gairdner had given a lecture at Edinburgh (1861) on cardiac murmurs, and spoke of the "auricular systolic murmur," but whether or not this was accepted I cannot say. At the time of my studentship the presystolic bruit now universally recognised was certainly not taught or spoken of in the medical lectures. Considering the time when Laennec lived and when the committee of investigation soon after was formed, it may be said

that Dr. Gairdner re-discovered the presystolic bruit thirty years afterwards.

I then allude to the case of imperfect aortic valves, and refer to the pamphlet written by Dr. Corrigan, of Dublin, in 1832; and as he had laid great stress on the character of the pulse, it was usually styled "Corrigan's pulse." I mention in my paper that Dr. Hodgkin five years before, in the year 1827, read a paper at the Hunterian Society on the subject of retroversion of the aortic valves, and was the first, I believe, to recognise this disease. He gives full particulars of the "*bruit de scie*" and of the elongated valve which hung down towards the ventricle; all the symptoms associated with this condition he clearly gives and explains.

Dr. Moxon has a paper on "Atheroma of the Arteries." His object is to show that this is a common name for patches found in the aorta and other vessels, usually regarded as a degenerative process, whereas it is often inflammatory and by no means degenerative. He further describes more fully this arteritis which is so often overlooked.

Then there is a paper by myself on "Adherent Pericardium." I mention the frequent occurrence of adhesions due to some previous inflammation, and which do not produce any influence on the heart's action; but on the other hand a universal adhesion of the pericardium leads very often to great impediment in the circulation, producing all the symptoms of heart disease, such as dropsy; but there being no marked alteration in the valvular sounds, a great obscurity attaches to all such cases of this

kind. In some instances where much breathlessness and lividity existed a malformation was suggested, especially if no bruits existed or any of the usual indications of valvular disease. In some cases where the pericardium was closely adherent and the usual symptoms of heart disease were present, all the valves were perfectly healthy. I mention also that in some cases the lung over the pericardium was also closely adherent. In others the inflammation had involved the larger vessels at the base of the heart and then extended to the mediastinum. I say "I would especially draw attention to the fact that not only does the inflammatory action very often involve the vessels entering the heart within the pericardium, but when the latter is adherent the thickening and induration proceeds as would a growth, to involve the cellular tissue in the mediastinum, and even creeping upwards to surround the veins in the neck. I have seen in two or three cases a tough fibrous or areolar tissue surrounding the vena cava and brachio-cephalic veins in connection with an old pericarditis." This is now called *mediastinitis*.

In a boy, aged 9 years, who died of cardiac dropsy, there was a universally adherent pericardium, which penetrated the muscular tissue of the heart; and this adventitious tissue was as thick as the wall of the heart itself, and on the exterior surface very hard. The valves were quite healthy, liver nutmeg.

A young man, aged 26 years, who had had cardiac symptoms for many years, suffering much from

dyspnœa, with blue and cold extremities, died at last rather suddenly. On post-mortem examination the pericardium was found universally adherent, and in some places an inch thick, and in others even thicker. This encroached on the muscle, and surrounded all the vessels coming from the aorta, as well as the trachea; in fact, all the parts in the mediastinum were involved in one hard mass of fibrous tissue (mediastinitis).

I have another paper especially to show the value of examining the brain in all cases, for not only may marked cases of insanity be shown in general paralysis, but in others there is often found considerable wasting of the brain, accompanied by much fluid on the surface, in persons where only a general mental weakness has been manifest.

Dr. Pye Smith has a paper on "Left-handedness," not to discuss its character, but why it should be exceptional and right-handedness the rule; whether there was any innate difference in the brain, that is, whether there might be any difference on the two sides of the brain, whether the right-handedness prevailed in all nations, or whether it might not be acquired. He left the question unsolved.

Vol. 17, 1872.—In this volume I have a paper on "Diseases of the Nervous System," and speaking of "aphasia," I allude to the phrenologists who first accurately described it, and also its seat as being over the eye; but all which the phrenologists did in the cause of cerebral physiology I shall leave

to another paper. I mention the case of a woman who had tetanus and *bloody sweat*. An analysis of the fluid was made by Dr. (Sir. Thos.) Stevenson, who found no red corpuscles in it, nor other indications of blood. I mention some animals who have a coloured secretion from the skin, and the story of Charles IX of France, who suffered agonies on remembering his order for the massacre of St. Bartholomew.

Vol. 18, 1873.—Dr. Fagge begins this volume by a paper on "Acute Dilatation of the Stomach." This subject was one to which the attention of the Profession was then being drawn, so that he was able to give several cases besides the one he now fully reported. They mostly occurred in young persons and occasionally were fatal. He discussed the causes or the pathology of the affection, which seemed very obscure.

Then follows a paper by me on some nervous diseases, dwelling more especially upon cases where an injury to a limb has been followed by wasting. I ask whether this is due to an injury to the nerves themselves, or whether this degeneration may not be carried back to the spinal cord at the place where the nerves arise. The question is suggested by two cases, where a limb wasted after an injury and then the arm of the opposite side became involved. I say I am quite alive to the possibility of the primary accident having affected the central organ, the spine.

This is followed by a paper by Dr. Debus, the lecturer on chemistry, on the "Artificial Formation of Organic Substances," showing the wonderful advance that chemistry had made during the last half century, and he quite expects that the future will produce chemists who will by the synthetic method make many substances which at present are only known as existing in the animal and vegetable kingdoms. He says that before the year 1828 it was believed that organic bodies could only be formed under the influence of the vital force existing both in animals and vegetables. In that year Wöhler made urea, and this was followed by some other chemists whom Dr. Debus mentions. This paper, it will be seen, was written in the year 1873.

If Dr. Debus had taken up the subject at the present time he would see that his prophecy was perfectly fulfilled, but he would also have added to his knowledge by the discovery of bacteria, which often play a large part in processes which were considered to be formerly purely chemical.

Dr. Moxon writes a highly logical paper on the "Nature of Tumours." He begins by saying that a very ordinary distinction is made between "innocent and malignant growths," that in the one case the growth is composed of the ordinary natural tissues and in the other of something altogether new or foreign. This he says does not bear criticism, nor many other definitions of which he shows the fallacies. He then points out what direction the study of cancer should take.

Then there is a well-argued paper by Dr. Thomp-

son Dickson on the "Dynamics of Catalepsy and Convulsion."

Vol. 19, 1874.—A remarkable case is described by Dr. Frederick Taylor under the heading, "A Case of Excretion of Urea by the Skin." It was that of a woman, aged 31 years, who was suffering from Bright's disease, and which after death was found to be of a very chronic nature, as it was due to an atrophied granular kidney. Besides other symptoms, such as inveterate sickness, she was covered with patches of a white substance; these were adherent to the skin, which made it look as if flour had been sprinkled over it. Under the microscope this material presented the appearance of small crystalline prisms. When removed and analysed, with some nitric acid added, characteristic hexagonal crystals of nitrate of urea were produced. This was confirmed by Dr. Stevenson. Dr. Taylor, in referring to foreign literature, found two or three cases of a similar kind, but these patients were suffering from cholera and typhoid.

In this volume is a paper by Dr. Galabin "On the State of the Circulation in Skin Diseases." This was attempted by means of the sphygmograph, an instrument only lately invented, and then very scientifically explained by Dr. Galabin.

Vol. 20, 1875.—Drs. Moxon and Goodhart on "Bacteria in the Blood," being then a subject of great novelty.

Dr. Galabin "On the Interpretation of the Cardiographic Tracings."

Then there is a paper by Dr. Moxon on "Insular Scleroses of the Brain and Spinal Cord." He describes several cases, and states that the two cases which he gives, although several had been described on the continent, were the first which had been published in this country, especially as the post-mortem appearances were thoroughly and accurately recorded.

They both occurred in young women. The spinal cord not only contained the grey-coloured plates, but the brain also had similar ones scattered throughout its substance. The paper is accompanied by a very good drawing showing these appearances, which consist of atrophied nerve-fibrils and new fibrous tissue, the colour being of the same hue as that of the cineritious colour of the brain.

Vol. 21, 1876.—Dr. Frederick Taylor has a report "On the Diuretic Action of the Resin of Copaiba," a preparation to which I drew attention in the 'Lancet' of 1873. He gives many cases of hepatic ascites showing its value.

Then there is a paper by Mr. Golding Bird "On the Treatment of Ulcers by the Application of a Weak Electric Current." He had already written his observations on the subject in a previous volume, as well as his father, Dr. Golding Bird. He placed a silver plate on the ulcer, and a zinc plate on a raw surface made by a blister. On connecting those by

a wire a destructive process commenced on the site of the new wound, while the original ulcer began immediately to heal. The artificial one, after taking off the plate, quickly healed also.

Then there is a paper in reference to a complaint which had been prevailing for some time, having already been observed on the continent. That is the case where a child had a membrane formed on the throat, tonsils, and perhaps on the larynx. Such cases had hitherto been called membranous laryngitis, to distinguish them from simple laryngitis. A discussion took place at the Royal Medical and Chirurgical Society with little agreement, one gentleman stating that the complaint might be simple laryngitis or membranous laryngitis, and if the throat and tonsils were covered by a membrane it was diphtheria.

Vol. 22, 1877.—I resume in this volume the subject of the formation of false membrane on the mucous surface of the air-passages, and especially of the bronchial tubes. Many cases had been described and published of membranous formations through the whole of the tracheal tubes; but now diphtheria had appeared as apparently a new disease, the question arose whether not only the membrane in the throat and larynx but in the bronchial tubes also should not likewise be called diphtheritic.

Then follows a paper written by "Dr. F. Taylor and Goodhart" to investigate the statement by Dickinson that in cases of diabetes he had found dilatation of the smaller blood-vessels in the brain

and spinal cord, with some extravasation around them, as well as some degeneration of the tissue. After a most minute and searching examination, these authors were unable to verify the statements by Dr. Dickinson in several fatal cases of diabetes.

Dr. Stevenson gives two cases of fatal poisoning by "phosphorus," and in which fatty degeneration of the liver was found and a less condition of the same change in the other organs; both patients lived only a few days.

I then have a paper, styled "Historical Notes on Bright's Disease, Addison's Disease, and Hodgkin's Disease." I speak favourably of these appellations, as they merely indicate the names of the men who first brought them under professional notice, without regard to their having been observed before, sometimes even by the ancients; and for another reason—they do not interfere in any way with a further investigation into their true pathology. So many workers have wished to foist their own views on the profession, and so, connecting their name with it, have given an appellation to their theory which to them is finality. Fortunately we did not accept George Johnson's "desquamative and non-desquamative nephritis" for Bright's disease, nor Dickinson's term "depurative" for "lardaceous" disease; nor a term signifying "born of putridity," invented by Dr. Murchison, as the final name for "typhoid." In the same way, the general expression "perityphlitis" is a better term than appendicitis, which was found to be the case in issuing the bulletin after the operation on the late King

Edward. Unfortunately a more general name might have been given to the case of atrophy of the thyroid body than "myxœdema"; even if Dr. Ord had discovered it, it would be better named "Ord's disease" than that very misleading term of "mucine disease," which is not an essential effect, but only one of the secondary ones. Fortunately no one had dared to change the names of smallpox, measles, or scarlatina for any supposed theory of his own as to their pathology.

As regards Bright's disease, both the urine being rendered opaque by heat and the altered state of the kidney itself had long been known, but it awaited the time of Bright before the two were associated and kidney disease to become then a well-recognised established malady. In the year 1795 Dr. Blackhall, of Exeter, had described a form of dropsy where the water was albuminous, as shown by its coagulation by heat, and long before that Morgagni had displayed the small granular kidney on the post-mortem table. This was at the University of Bologna, where he had been himself a pupil of Valsalva, who was sixteen years his senior. I might mention that Valsalva had been a pupil of Malpighi, who had been professor at Padua in the year 1715, and I believe it was in this year that Morgagni was applying for the chair. I state in my paper that Morgagni says in his chapter on diseases of the kidneys that "they are sometimes found small and uneven on their surface owing to hemispherical protuberances." In another part of his book he says "a controversy was going on about

the red colour of the urine in a certain abbot, when he (Morgagni), recommended that fire should be applied, and if it was blood it would coagulate." It had been for a long time taught that the serum in dropsy would pass into the blood and this again into the urine and so carry some of the blood with it, and might be coagulated by heat. He noticed that patients with suppression of urine had convulsions and vomiting; also that the detention of a large quantity of the urinary matter in the blood proves fatal through its deposit in other parts of the body. He states that this was what Malpighi taught, and that in one case where there was disease of the kidneys, the latter mentioned that the patient spat up saliva which tasted and smelt of urine. Morgagni had had a long experience, and when eighty years of age published his celebrated work, entitled "De sedibus et causis morborum per anatomiam indagatis," evidently meaning "Concerning the seat and causes of disease diligently sought for by means of dissection." This reminds one of Harvey's instruction to "search out the secrets of nature by way of experiment." Morgagni lived ten years after this and died in 1771, aged ninety years.

With regard to "Hodgkin's disease," I need not say more than has already been stated in these 'Reports' and alluded to in the present outline of their contents, especially in vol. ii of Series III for 1856.

"Addison's disease" stands out as a far more unique disease and personal discovery than the

other two, as the small bodies known as the supra-renal bodies had been totally unobserved in either their natural state or diseased condition before Addison's time.

I need not here repeat what I have already said as to its early history and discovery by Dr. Addison, as it is to be found in connection with the description of cases in vol. v Series III for 1859.

Vol. 23, 1878.—In this volume Dr. F. Taylor has a paper, entitled "Contribution to the History of Idiopathic Anæmia or Pernicious Anæmia." I have already alluded to the fact (in the 'Guy's Hospital Reports,' 1857) that the latter term, by which the disease is usually known, was applied to a condition which had already been described and lectured upon in the ordinary course by Dr. Addison. At this time I made several post-mortem examinations of these cases, and found no disease but marked fatty degeneration of the heart. Dr. Taylor's paper confirms all that I said in reference to the originality of Addison's observations, and "that the disease, described by a German physician under the name of 'progressive pernicious anæmia,' is no other than the 'idiopathic anæmia' which found a place in Addison's lectures nearly forty years ago." He alludes also to the fact that it had already been recognised in England, and Addison himself had alluded to it in his work on "Disease of the Supra-renal Capsules."

In this volume I make mention of a number of

unpublished papers by Dr. Hodgkin, which he wrote forty years before, when he retired from Guy's. I may in this place remind the reader that Dr. Hodgkin was the first Curator of the Museum and Professor of Pathology. He was born at Tottenham in 1798, took his degree at Edinburgh, and became a member of the London College of Physicians in 1825. In the following year, when the new school of Guy's was opened, his appointment was made. No one worked harder to accomplish what he said his endeavour was to do—"to make the school of Guy's one of the first in the kingdom." He resigned in the year 1837, after having failed to be elected to the vacant Assistant-Physicianship. He died and was buried at Jaffa in 1866, aged 68 years.

As pathology was almost a new subject taught in the medical schools, he placed in the museum all objects of interest which he found, although not aware of their true nature. He put up specimens of lardaceous disease, which, he said, "cut like bacon, and had a peculiar translucent and unnatural uniformity of structure." Then specimens of "enlarged glands and white deposit in the spleen," the disease which now bears his name; also a well-marked specimen of typhoid disease of the ileum, which he calls scrofulous. He used a microscope in conjunction with his friend Lister, and read papers on what he saw. "The whitish corpuscles in the blood are intimately connected with the lymphatic system, for I have found them particularly conspicuous when the lymphatic glands throughout

the body were remarkably large." He also speaks of the difference between the structure of the ordinary voluntary muscles and that of the œsophagus and bladder. He alludes also to experiments on animals and the results which have been obtained, apologising for not having made them himself, saying that their performance would have been too painful for him to undertake. I believe the specimen of "Trichina spiralis" in the museum was the first that was ever preserved; it was termed "a portion of sterno-mastoid muscle, speckled with numerous minute bony particles." He speaks of these having been found by his assistant, Mr. H. Peacock, and when afterwards a similar condition was found by John Hilton in the dissecting-room, they were referred to Professor Owen, who found that they were of a worm-like nature, and gave them the name of "Trichina spiralis."

Dr. Hodgkin read a paper at the Hunterian Society in February, 1827, entitled "Retroversion of the Aortic Valves." He commenced by saying that this had not been mentioned by Corvisart, Laennec, Rostan, Bouilland, Andral and others; he then described how an aortic valve might be rendered inefficient and so allow the blood to flow back again into the heart. As there was violent inordinate arterial action, the ventricle became hypertrophied and caused a remarkable thrill in the pulse, which, however, was regular. There was a double sound or bruit, as both the systole and diastole of the heart were accompanied by one. He exhibited the heart at the Society, which showed one valve retroverted

and hanging down into the ventricle. The specimen is now in the museum. It was in the year 1832 that Corrigan, an Irish physician, wrote his paper in the 'Edinburgh Medical and Surgical Journal' entitled "Inadequacy of the Aortic Valves." Corrigan's name was given to this very characteristic pulse.

It was at one of these meetings of the Hunterian Society in March, 1829, that Dr. Babington described his newly invented instrument the "laryngiscope," or "glottiscope." The report says that "Dr. B. Babington submitted to the Society an ingenious instrument for the examination of parts within the fauces not admitting of inspection by unaided sight. It consisted of an oblong piece of looking-glass set in silver wire with a long shank. The reflecting portion is placed against the palate, whilst the tongue is held down by a spatula, when the epiglottis and upper part of the larynx become visible in the glass. A strong light is required, and the instrument should be dipped in water, so as to have a film of the fluid upon it when used, or the halitus of the breath renders it cloudy. The doctor proposed to call it *glottiscope*."

Dr. Hodgkin refers to it in a lecture as the *speculum laryngis* or *laryngiscope* invented by my friend Dr. Babington in 1829. Dr. Hodgkin speaks also of the "evolution of species." He alludes to the various transitions in the development of the fœtus; for example, how the heart in its progress exhibits those forms which are permanent in many. There are instances, he says, in which the structure

of the fish and batrachial reptiles is preserved. He speaks also of the similarity in the extremities of all vertebrated animals, made after one model or type, but modified according to their use. He says, too, that the production of new species of animals of the higher classes has not been limited to one spot or one period of time; also the evidence of the creation of animals having taken place at different time is equally conclusive. The researches of the geologist have shown us the remains of numerous animals once inhabiting the earth's surface now extinct, and that those animals with which we are at present acquainted, with some few exceptions, did not exist.

Another paper by Dr. Hodgkin was an essay on "Oxford's" trial. Oxford was a young man who was confined in Broadmoor Asylum all his life for shooting at Queen Victoria. The doctor discusses the question, then much in vogue, whether there was such a condition as might be called "moral insanity" in contra-distinction to intellectual insanity, the law only recognising the latter—that is, the knowledge of the difference between right and wrong. Hodgkin argues in favour of such a state as moral insanity, or, as the French call it, *lésion de la volonté*.

Then he has a paper entitled "Physiological Remarks on Language."

Another on "Medical Theories," in which he speaks of the time when he studied at Edinburgh, and the professors begun their course by lecturing on "fever," and discussing the various opinions of

its nature according to the theories of Cullen, Brown, and others; these depended mainly on their views whether the fluids or the solid organs were most at fault. Then he spoke of the French "medicine experimentale." He hoped, however, that the time would come when all these theories would be put aside and a truer pathology take their place, founded on a true knowledge of the different changes in the organs and other tissues.

Vol. 24, 1879.—Dr. Goodhart has a paper on "Acute Dilatation of the Heart as a Cause of Death in Scarlatinal Dropsy." He gives several cases of children at the Evelina Hospital who had scarlatina and albuminuria and died suddenly, when the cause seemed to be due to dilatation of the left ventricle; this was often most marked whilst no indication of pericarditis existed. He alludes to an observation which Sir William Gull had already made in respect to this, that sometimes death might take place in scarlatina which was erroneously attributed to pericardial effusion, whereas it turned out to be acute dilatation of the ventricle.

In another paper Dr. Goodhart discusses the "Ætiology of Scarlatina in Surgical Cases." He mentions several which he saw at the Evelina Hospital, and there the question of contagion could not be excluded; so it seemed to show that there was an extra susceptibility for a child to take the disease who had undergone an operation. This had been shown also at Guy's, where the children's surgical

ward had more than once had scarlatina quickly run quite through the ward ; on the other hand there are surgeons both in England and on the continent who have frequently observed a rash after operation, and which they have described as merely resembling scarlatina. Then I have a paper entitled "Notes on the History of the Nervous System, taken more especially from Writers on Phrenology." When I happened to take up some volumes of 'Transactions of a Phrenological Society,' I was not only astonished but edified in reading through their pages. It gave an account of Gall, I believe a German, and how his colleague Spurzheim came to London in 1825 to give some lectures on the brain. The journal says "he (Dr. Gall) has a superior mode of dissecting the brain, not like that practised in most anatomical schools, by mere slicing off large portions of the brain, but by following the more natural way of tracing the course of the fibres. In the year 1804 Gall had already dissected the fibres of the pyramidal bodies and describes the decussation and also the tranverse fibres of the pons Varolii and so on, until he reaches the hemispheres, which he regards as the seat of intellect and sentiment. As regards the spinal cord, he does not believe it is a mere conductor to the brain, but it has some function of its own, especially remarking its enlargement where the limbs are given off."

Then, again, the phrenologist had long settled the use of the double brain, which was still puzzling the physiologist, as to its meaning, for the duality of the brain was still a term in use and a problem

when I was a student. The standard work then was 'Carpenter's Physiology,' which spoke very doubtfully as to its meaning, discussing whether one side was the seat of morality and the other of vice and similar imaginary theories. The phrenologists who first looked upon the brain as a material organ at once regarded its two sides as intimately associated with the two sides of the spinal cord, and therefore closely connected with the limbs.

Then in these transactions again came the seat of language in the frontal lobe, and a good description of "Aphasia," Gall believing that the brain was the organ of all the mental functions, and observing that his fellow-students who were most ready to speak and acquire the power of learning language had the most prominent eyes, he regarded the part of the brain above the eye as the seat of language. This was confirmed by noticing cases where a stick or other object was thrust into the eye, and, piercing the brain, produced "aphasia." This is well described by showing how the patients knew the words but could not speak them. I should say that there is no intimation of the phrenologists knowing that injury to one side would produce this loss of language more than the other. This article on aphasia is certainly most noteworthy, for the cases so well described are the first that I ever read in print, and reported long before those in ordinary English works. However superficial might have been the reason for the phrenologists believing that the organ of speech was over the eye, they were certainly most accurate in describing the symptoms

which accompanied such injury. Gall said "the power by which we employ signs to represent our ideas and feelings is connected not only with the anterior lobes of the brain, but with that portion of those lobes which rests on the centre of the orbital plate; the manifestation of verbal language depends on a cerebral organ, and this cerebral organ lies on the posterior part of the superior orbital plate." Several illustrations are given in the 'Edinburgh Phrenological Journal' volumes, one a striking case brought before the society in 1832, in which a girl had received a gunshot wound in the orbit. Dr. Inglis stated that "she knew everyone and understood what was said to her, but appeared to have lost her memory for words." In some cases where a post-mortem examination was made (1822) there was found "in the centre of the under-surface of the anterior lobe (viz. in the situation of the organ of language) the convolutions to the extent of half-a-crown were changed in colour to a light reddish-brown." At a later date when a case of aphasia was taken before the Royal Medical and Chirurgical Society of London (1838) a member thus expressed himself: "If a person could move the tongue and the voice was present, he must be an impostor if he did not speak."

Another case I read in a French journal is that of a "French soldier who was struck by a bullet at the battle of Waterloo on the outside of the forehead, six or eight millimetres from the left eyebrow, and at a point corresponding to the curved line in the temporal fossa. This man eventually recovered,

but he never regained the memory of proper names and of some substances, although his intellectual faculties were unimpaired. He died eventually of phthisis, and Baron Larrey showed the skull, together with the ball, at the Academy of Sciences." It seems as if aphasia was recognised by several French physicians, as Bouillard, Majendie, Moneau and others.

Vol. 25, 1880-81.—In this volume is a very interesting and important paper, entitled 'Chronic Bright's Disease without Albuminuria,' by F. A. Mahomed, M.D., one whose early death was a great loss to the profession, possessing as he did great power of insight and of originality. His thesis was to the effect that morbid anatomy or the gross disease which is discovered on the post-mortem table is not necessarily the foundation of the ailments which we have. For example, diabetes might turn out to be a disease of the nervous system. Nothing was more common than to see cases of kidney disease with marked albuminuria associated with disease of other organs, and this we considered sufficient warrant to regard the albuminuria as the prime affection. Dr. Mahomed then gives several cases where he found great arterial tension, and he thought that in this increased blood-pressure might be discovered the cause for the albuminuria, as in various diseases where a high pressure is shown during life. A consequence would be albuminuria, and even disease of the kidney itself, and perhaps

of other organs also, might be found. He then reports many cases where he found high arterial tension. He read a paper on this subject at the Royal Medical and Chirurgical Society, which can be found in the 'Transactions' for 1874. It is entitled "The Ætiology of Bright's Disease in the Pre-albuminuric Stage."

This opinion of Dr. Mahomed is held most strongly by those who have had the greatest experience of morbid anatomy. One well-known physician (Dr. Sturges), holding the opinion that chorea was a general disease and not a local one, as held by another medical man, took for the motto of his work on Chorea: Τί ζητεῖτε τὸν ζῶντα μετὰ τῶν νεκρῶν; "Why seek ye the living among the dead?" Another example of a like opinion was that of Dr. Sutton, Physician to the London Hospital. In his introductory lecture to his course he says: "Although the first step in the examination of the process of disease is to study morbid anatomy, yet we must not go through life seeing all disease through the atmosphere of the dead-house, for morbid anatomy teaches only this—what disease has accomplished; what we see on the post-mortem table is past and gone." In another place he says: "We must find morbid anatomy to love it."

Vol. 26, 1881-2.—This number contains a paper by Dr. Peter Horrocks on "Reflex Action as an Aid to Diagnosis." It was about this period that the subject of reflex action in connection with

diagnosis was coming into vogue, and so Dr. Horrocks explained the methods of using it by describing the different anatomical parts with the physiological functions of those regions where these reflexes were more commonly put in practice.

In this volume is a paper by Dr. Frederick Taylor on "Diabetic Coma." He gives numerous cases, and discusses the question of the causes of this form of death. This is followed by an interesting article by Dr. Francis Galton, F.R.S., and F. A. Mahomed, M.D., entitled, "An Enquiry into the Physiognomy of Phthisis by the Method of Composite Portraiture." Some hundreds of cases of this disease were taken by photographs at Guy's Hospital, Brompton, and Victoria Park Hospital. These were divided into three lots, and each of these was set moving rapidly over one sensitive plate, so that finally one photo was formed which was a compound of all. Mr. Galton thought that in this way he should obtain a marked portrait or representation of a phthisical patient, and so perhaps of other marked diseases. The result, however, was very different from the expectation, for none of the morbid characters were apparent but had altogether gone, leaving only an ideal inanimate face, every individual peculiarity having disappeared.

In this volume I have a paper on "Hemianæsthesia," so often noticed in connection with hysteria. This is so frequently combined with paralysis of motion that a certain amount of loss of both motion and sensation may be regarded as existing in all cases which pass by the name of hemianæsthesia,

including also a failure of all the special senses. It is, in fact, a sudden abeyance of the functions of the entire hemisphere, and so very different from the ordinary hemiplegia arising from an actual disease of definite portions of the brain. Here the hemiplegia is usually not complete, as it does not include the whole of the face nor affect the special senses. I continue, then, to speak of the necessity of admitting the existence of such a condition as the brain being structurally healthy and yet not functionising. We have only to think of a lad, the native of some uncivilised country, being taken to Europe to be educated for two or three years, and then on his return being compared with his twin brother. The two would be outwardly alike both in features and configuration of head, yet as regards their brains, even if they could not be physically distinguishable, yet must be totally different as to their action and working powers. In a similar way a man in deep thought may be walking through the crowded streets and be quite unaware of objects about him, and yet his spinal system must be still at work so as to prevent him running into any danger. The police have often come across such persons in the streets, and I mention one of the kind brought to me at the hospital. I also mention the case of hemianæsthesia in two men, who, if they had been of the other sex, would have been regarded as suffering from hysteria. One of these patients affected to be paralysed, and was cured at the famous shrine at Lourdes.

Vol. 27, 1883-4.—This number contains a memoir of Mr. Joseph Towne, the distinguished modeller in wax, who had lately died. It mentioned how he first presented himself to Sir Astley Cooper, with the model of a skeleton in wax. Sir Astley was much struck with it, as the youth was only seventeen years of age, and sent him to Mr. Harrison, the treasurer of Guy's. Mr. Harrison soon after engaged him for similar work, especially to copy the dissections made by Mr. Hilton. His appointment dated from 1826, and he remained attached to the Hospital all his life, for nearly half a century. For the skeleton he obtained a gold medal at the Society of arts in the same year, 1826; he subsequently obtained other prizes, as in the French Exhibition. This is followed by an article supplied by Mr. Jacobson, "On Growth in the Face and Neck." He mentions also one of a similar kind described by Mr. Birkett, in vol. ix of the 'Pathological Transactions.' The latter surgeon said he believed they originated in opercula in the second or third visceral arch, and quotes Cohnheim's opinion that many tumours are the relics of fœtal tissue which for some reason have not undergone full development. The same reason accounts for supernumerary auricles and fistulæ, both aural and cervical, such as Sir James Paget has reported in the 'Medico-Chirurgical Transactions' as cases of "branchial fistulæ" in the external ears.

In this volume Dr. Pye Smith gives a very elaborate report of a case of "Idiopathic Anæmia,"

followed by comments on the history of the disease. He says that Addison was the very first who gave a good account of this remarkable malady; cases of it might have occurred and come under the notice of different medical men, but it had never been mentioned before as a distinct disease. Dr. Pye Smith relates how Addison fully described it in his lectures, and afterwards at the "South London Medical Society," the report of which was published in the 'Medical Gazette' in connection with his account of disease of the "supra-renal capsules." This was in March, 1849. This affection spoken of by Addison was well-known to his pupils, and to medical men whom he might meet in consultation about a case of the kind. In London, Dr. Quain, Dr. Bristowe, Dr. Sutton, were quite familiar with his account of it, and in Canada Professor Howard spoke of Addison's "Idiopathic Anæmia" in his lectures, so the complaint was fully recognised.*

In the year 1871 Professor Biermer described some cases of the same kind and called the disease "progressive pernicious anæmia." In consequence of this the disease became well known in Germany, and the name appearing also in the English journals it became quite familiar to their readers. Indeed, so little was known abroad of Addison's perfect description of the disease that most of the profession regarded Biermer's discovery as perfectly

* When I was assistant physician I showed two young German doctors round the hospital, and amongst other cases pointed out a case of idiopathic anæmia. They were sceptical, and said if they had had time they should have liked to have examined the patient themselves.

new, and so the usual name became "pernicious anæmia." I follow Dr. Pye Smith's account by saying this affection, always having been regarded with great interest, had not only appeared under this last name in most medical works, but some authors had fully discussed the subject, and also whether Addison's idiopathic anæmia was the same as Biermer's progressive pernicious anæmia. Some of the German authors allude to Addison's knowledge, but they dismiss him rather summarily as if he had only a superficial, imperfect view of the disease, whereas the German physician gives a more thorough and detailed account of it and first named it. Since this time further investigations have been made and the modern method of research has been employed in order to show whether any differences really exist between these two forms. Dr. Pye Smith then quotes a paper by me in the 'Guy's Hospital Reports' for 1855.

Dr. Hunter has written extensively on the subject and gives both Addison and Biermer credit for having made independent discoveries, and he is inclined to think that the two are different in many ways, especially as regards the blood from Addison's form being infectious, so he proposes to call these "Addison's anæmia" and "Biermer's anæmia." It appears that in the German professor's account he alludes to the frequent hæmorrhages in the retina. This has been subsequently found to be the case, but it is to be remembered that in Addison's time Helmholtz had not invented the ophthalmoscope. However different these two forms of anæmia may be, as

regards their general symptoms they are much alike, and that in all probability most practitioners will regard Addison's anæmia and Biermer's anæmia as the same disease, and so probably the term "pernicious anæmia" will generally be adopted.

This volume contains a memoir of Dr. Hilton Fagge, who died in 1883 at the age of forty-five; also an article by Dr. Hale White on the question of the "Theory of a Heat-centre" in the nervous system. To this biography of Dr. Fagge may well be added his share with the late Sir Thomas Stevenson in introducing the physiological tests for many vegetable poisons which could not be discovered by the ordinary chemical means. He and Stevenson had been making experiments on the action of digitalis, and insisted on this test in the detection of poisons. The importance of the method was soon afterwards recognised when the judge accepted the evidence obtained in this way. Soon after the publication of the trial there appeared a French novel, called 'Le Docteur Claude' by Hector Malot. In this story the heroine is poisoned by a vegetable substance, the presence of which in the body could not be ascertained by chemical analysis, whereupon an expert declares that it might be discovered by another method, called the physiological test—that is, by observing the effect of the vomited matters and extracts of the tissues upon animals: "'Elles sont de vos expériences, monsieur le pharmacien?' demanda le président. 'Non, monsieur le président, elles sont de Fagge et Stevenson.'"

Vol. 28, 1885-6.—This volume contains a memoir of Dr. Mahomed.

Then follow other communications by numerous members of the staff who had now reached the age and position of becoming my colleagues. The first of these are by Mr. Arbuthnot Lane, who commenced to employ his observing and scientific mind immediately he was placed in the dissecting-room, and before the time when others did the same, after obtaining practical experience in the wards. These later volumes contain equally valuable contributions as those in the earlier volumes, but my object in noting those was to mark the commencement of scientific medicine and pathology, indeed as giving points in their history. As regards Mr. Lane, he observed how the framework or skeleton varied according to the occupation in which the man had been employed; whether he had been a porter and carried weights on his shoulder; a blacksmith; or a cobbler. Other modifications as regards the joints he also noticed.

As I have already said, my object in writing this memoir was not to retail to my readers my own biography, but rather to state the numerous medical facts, now well known and recognised, which came into existence, or, rather, were first recognised, in my own student days. With this object I thought I could not do better than look through the 'Guy's Hospital Reports' to ascertain what I required, because these reports were the first issued from any London hospital, and it was expected also by

their editors that they would place therein any novelties or cases which occurred in their wards and had not before been described. The first volume was issued in the year 1836. This selection I have made, and now will merely draw attention to those cases which seem to have fulfilled my object and appear to have been not before described. I might first allude to the three diseases which are named after three of the Guy's physicians, viz. Bright, Addison, and Hodgkin, although there are other maladies which they seem to have been the first to describe. As regards Bright, the first appearance of this newly observed kidney disease is to be found in his 'Medical Observations'; but the whole *resumé* of the nature of the malady and the symptoms are to be found in the essay I wrote in the volume for 1877 of the 'Reports,' this being founded on what I had learned from my teachers, some of whom were Bright's colleagues.

As regards Addison's disease of the suprarenals, I would mention that the volumes for 1859, 1862 and 1865 are where the account can be found; also in connection with Addison's idiopathic anæmia a description may also be found in the volumes for 1857–58, and also under the name "Pernicious Anæmia" in 1878, more fully given in 1885–1886.

As regards Hodgkin's disease, it is mentioned by Bright as early as 1839, vol. iii of the 'Guy's Hospital Reports,' and more fully in the year 1856.

In the first volume, Bright's essay on epilepsy should be remembered as an introduction to what is now known as Jacksonian epilepsy. Also the

essay on the excision of joints, that on the thyroid gland by Mr. Wilkinson King, and above all, that on acute atrophy of the liver by Dr. Bright. In the second volume there are original papers by Dr. Bright on hydatids, and by Mr. Aston Key on lithotrity. In the third volume, containing a paper by Dr. Bright on the spleen, he refers especially to the malady described by Dr. Hodgkin. In the fourth volume for 1839 Dr. Hughes describes cases now called "embolism." In vol. v for 1843 is Dr. Chevers's paper on the subject afterwards called "pyæmia"; also an account of puerperal epilepsy, by Dr Lever, and its cause. Then I may draw attention to the volume for 1849, containing an original paper on "Adenoid Tumours" of the breast by Mr. John Birkett. Then in the volume for 1850, by Mr. Bransby Cooper, on the use of the microscope in ascertaining the nature of both healthy and morbid structures. In the volume for 1852–3 there is a further mention of unilateral epilepsy, also of typhlitis, and a quotation from a German encyclopedia in reference to Bright's disease. In the volume for 1858 is an original paper by Gull on disease of the spinal cord, and the first operation of gastrotomy by Mr. Cooper Forster. Then, in 1860, is Durham on sleep, and another on hermaphroditism. Then in the volume for 1863 is my paper on syphilis of internal organs, and in that of 1878 a more complete account of pernicious anæmia.

Now I have gone through the 'Guy's Hospital Reports' in the past years in which I had a knowledge of them, I will refer to some other diseases which

came before the notice of the profession at the time I was working and making pathological observations. I will refer again to the history of unilateral or Jacksonian epilepsy, to acromegaly, osteitis deformans, acute Hodgkin's disease or status lymphaticus, furrows on the nails, trichinosis, verruca necrogenica, and perhaps some other minor diseases.

I may mention first the special work I have done by giving bibliographically an account of all the subjects I remember and the journals which contained them.

My book on 'Pathological Anatomy' was first published in the year 1869, from the subject of the lectures which I gave in the years 1857–1858. For a second edition I had as collaborator Dr. Moxon, and this was written in 1875.

The third edition after Dr. Moxon's death was written by myself alone in 1889.

My book on 'Diseases of the Nervous System' was first published in the form of lectures in the 'Medical Times and Gazette' of the year 1868. These were published in one volume in 1878; a second edition in the year 1883. The lectures on "Specific Fevers and Diseases of the Chest" were published in the 'Guy's Hospital Gazette.'

The 'Journal of Mental Science' contains a paper by me called the "Study of the Human Mind" in the year 1875; a "History of my Parrot" in the year 1879.

'The Journal of the Association for the Advancement of Science by Research' for the year 1882 contains a paper by me on the "Value of Experiments."

The 'Guy's Gazette' contains the following reports: The Guy's Festival Dinner at the Imperial Institute, June, 1896; my election as President of the College, April, 1896; my biography, with portraits, 1893; complimentary dinner on Baronetcy, October, 1897.

'Brain' contains the following: "On the Pupil of the Eye in Emotional States"; "Sleep-walking and Hypnotism"; "How the Blind Dream"; "Hunter and Shelley."

The 'Medical Magazine' a monthly journal, published 1892: "The Functions and Powers of the Medical Council"; "The Science and Art of Medicine," January, 1901; "The Relations of Medicine and Surgery," June, 1903; "The Importance of Studying the History of Disease," November, 1901; "The Origin of Music," January, 1904; "The Nature of Dreams," 1904.

The 'Practitioner' contains the following: "Fifty Years Ago," 1897; "Treatment of Consumption—Review of Dr. McCormac's Book," June, 1898; "Treatment of Pneumonia," February, 1900; "Alcohol as a Beverage and Medicine," November, 1902.

The following addresses were published separately: Introductory address at Guy's, October, 1860; address at Birmingham, in 1872, on the occasion of the Annual Meeting of the British Medical Association; address at Cardiff on a similar occasion in 1885; address at the Midland Medical Society; address at the Pathological Section at the international meeting in London. My "Harveian Oration" was given in 1879; the 'Biographical History of

Guy's Hospital,' by Wilks and Bettany, was published in 1892; 'Chambers's Journal' published my lecture on "Overwork," delivered before the National Health Society in the year 1880; also another on "Temperaments" in 1876.

I may mention an address which I gave at Folkestone on October 1st, 1892, before the Church Congress on the subject of vivisection (see the 'Report'). I made this thesis the basis of a little book which was afterwards published by Mayle at Hampstead, in 1908, called 'The Relation of Man to Animals.'

I had been in the habit of keeping the medical journals in which I had written anything of interest, and these I will now mention for the sake of any friend into whose hands the present memoir may fall.

The papers in the 'Lancet' are as follows: "Effects of Alcohol," reprinted in a separate form; Lecture at Oxford called "Stray Thoughts," November 24th, 1894; "Scientific Therapeutics," February 18th, 1871; "Indiscriminate Use of Alcohol," April 27th, 1867; "Short Memoir of Oliver Wendell Holmes," July 3rd, 1886; "Homœopathy and College of Physicians," January 7th, 1882; "Psychic Force," November 4th, 1871; "Memoirs of James Hinton," January 8th, 1876; "Arsenic in Anæmia," April 11th, 1885; "Hypnotism," May, 1890; "Influenza and its Effect on the Heart," April 28th, 1890; "On the Value of Guarana," September 27th, 1872 (see 'Aberdeen Medical Student' for November, 1872); "On the Development of the Stethoscope"; "Addison's

Disease," July 28th, 1900; "Congestion of Lungs," June 18th, 1890; "Blood-letting," May 23rd, 1891; "On Syphilis," reprinted 1867.

From the 'Medical Times and Gazette': "Tubercular Meningitis in Adults," September 12th, 1863; "Rational Treatment of Disease," March 29th, 1862; "Virchow's Theories," July 12th, 1862; "On Hunter," April 25th, 1865; "Perforation of Stomach," with Dr. Barlow, 'Medical Gazette,' March 20th, 1845; "Furrows on Nails," 'Pathological Transactions,' March 20th, 1888; "Perils of the Sea Shore," my adventure at Scarborough.

Unilateral Epilepsy, now called Jacksonian Epilepsy.

It is interesting to follow the development of the history of this form of epilepsy as we understand it from its very first recognition as far as I can ascertain. This is to be found in the very first article which Dr. Bright contributes to the 'Guy's Hospital Reports' on its first publication, in the year 1836. In this paper he discusses the subject of epilepsy dependent on a local cause, and gives the case of a man, aged 37 years, under his care in July, 1835, who had a constant succession of convulsions; the patient showed many scars on his body which were of a syphilitic character. Then Dr. Bright further says: "The whole progress and seat of the disease is full of interest, and my reason for supposing the epileptic attacks in this case were dependent rather on a local affection than on a more general state of cerebral circulation or excitement was *the degree*

of consciousness which was observed to be retained during the fits; for although we meet with great variety in this respect, yet in two cases which have occurred to me the patient remaining conscious was a remarkable feature, while in each the injury on which the fits depended was of a local rather than a constitutional character. The present is a third instance in which I have observed this peculiarity in the state of the mind during the paroxysm; indeed, the symptoms taken as a whole in this case most strongly resemble those of the second I have mentioned, viz. the temporary paralysis of one side, the epileptic character pointing to the membranes and surface of the brain as the parts most affected."

These are the more essential portions to be quoted with respect to this variety of epilepsy.

In the "Clinical Report" for 1853 I mention the case of a girl who had frequent convulsive fits, and owing to that circumstance they were supposed to be due to a local cause; the brain after death was found to have in it several small tumours.

It may be as well to allude to the theory of the function of the brain which was held at the time I was writing. It was believed that the spinal tracts, the one conveying the motor fibres and the other the sensory, began or ended as the case might be in the corpus striatum or thalamus opticus, and that the impulses to the first came from the grey matter, the seat of the mental activities, and the same grey matter received the sensory impulses. It was known that irritation of the cineritious surface would produce convulsive movements, as so frequently observed

in tubercular meningitis. It also had been frequently observed and reported especially by Jonathan Hutchinson that an arachnitis on one side was often accompanied by a partial hemiplegia, but, as it always occurred from an injury, the paralysis was explained away by believing that the injury might have proceeded as far as the corpus striatum although there was no visible trace of it.

This view was the one generally held, as may be seen by a lecture given by Dr. Hughlings Jackson and published in the 'London Hospital Reports' for 1864: "The optic thalamus is supposed by some eminent physiologists to be the chief focus of the sensory nerves. The sensory tract may be traced upwards until it almost entirely spreads itself through the substance of the thalamus. The roots of the optic nerve pass into these ganglia and the olfactory lobes have a connection with them by the fornix, and it has been supposed by some physiologists that the corpora striata stand in the same relation to the thalami optici as the anterior horns of the grey matter of the spinal cord do to the posterior horns, the corpus striatum being considered a motor and the thalamus opticus a sensory centre. Medical physiology does not appear to harmonise strictly with these conclusions."

Soon after this time a German physiologist made some experiments on living animals, and found that an electric current excited certain convolutions to produce definite movements of the limbs. These were repeated by Dr. Ferrier, and his results, which were confirmatory, were published, some notes of

which I have still by me. Physiologists say that the motor centres of the limbs are in the convolutions bounded by the fissure of Rolando; those for the leg in the posterior parietal lobule and the upper part of the ascending parietal convolution; the hand and arm centres in the ascending parietal and upper ascending frontal; others for the lateral movements of the head for speech, movement of the eyes, etc.

Dr. Hughlings Jackson, who had already given much attention to the question of the different convolutions being associated with muscular movements, now commenced the further study of the question and arrived at many remarkable results. It is curious to observe that at the same time Dr. Russell Reynolds could place in his 'System of Medicine' his article on epilepsy, in which he says that considering all the theories about its seat and nature, he believes that the seat of the primary derangement is in the medulla oblongata and upper part of the spinal cord. I have already said that Dr. Hughlings Jackson had been studying and elaborating the connection between the cerebral convolutions and certain muscles and their movements. More especially could he do this in the cases of epilepsy where the movements were on one side only due evidently to a local cause and where the patient did not become insensible—the class of case first described by Dr. Bright many years before in 1836. This was so carefully studied by Dr. Hughlings Jackson and so clearly marked out as a special form of disease that it well deserves the name "Jacksonian

epilepsy." It is more interesting from the fact that the special movements indicate the particular convolution involved, and so enable the surgeon to locate the spot if a removable tumour is suspected.

Whilst on this subject I might allude to the early use of bromide of potassium in epilepsy. It had been first used in England by Dr. Locock as a sedative in the cases of women when hysterical or with other nervous affections, but as far as I know was not used for simple epilepsy and not at all for men. Wishing to try the bromide against the iodide I was surprised to find that the former had a power over the epileptic fits, and these were published in the 'Medical Times' when Mr. J. Hutchinson and Dr. Jackson were the reporters for the journal. This is so said in the 'Journal of Mental Science' in an article by Dr. Thompson Dickson, in which he mentions Dr. Wilks first using bromide in epilepsy. I remember Dr. Ramskill calling on me and informing me of the remarkable success he had had in a marked case of the disease.

Osteitis or Osteoporosis Deformans.

In the 'Lancet' of November 13th, 1909, was an annotation on the subject of "Osteitis Deformans," in which it is stated that Sir James Paget first described that chronic form of inflammation of the bones which bears his name. This refers to a paper which he wrote in the 'Medico-Chirurgical Transactions' for 1876, where he mentions this rare disease and the first which had been under his care. To this he added some others, one being mine which I had

taken to the Pathological Society in the year 1869 under the name of "osteoporosis."

My case is there fully set out as that of a patient who was under the care of Dr. Albert Massey, of Camberwell. It is carefully described as regards the great increase of size of all the bones, which were also bent, so that he had when standing more the appearance of an ape than a man, and the skull also was much hypertrophied. When he died, which occurred from his increasing inability to move the chest, I asked Dr. Goodhart, the House-Physician at Guy's, to assist me at the post-mortem examination. The patient's friends, knowing the case was a rare and important one, gave me *carte blanche* to take away some of the bones for further examination. This was done, and a very accurate and minute account may be found in the 'Pathological Transactions.'

I state in my letter to the 'Lancet' that, not finding any periostitis, I preferred the name "osteoporosis deformans." I might say that previous to this Mr. Durham had written a paper in the 'Guy's Hospital Reports' for 1864 on "Mollities Ossium," in which he gives a similar case.

Acute Hodgkin's Disease or Status Lymphaticus.

In the last edition of my 'Lectures on Pathological Anatomy,' published in 1889, under diseases of the "Lymphatic Glands" I have the following: "We have met with two cases of acute Hodgkin's disease in young lads. The disease lasted a few weeks and

was accompanied by febrile symptoms. The cervical and other lymphatic glands were enlarged to about the size of peas, and the liver, which was much enlarged, contained, like the spleen and kidneys, lymphatic deposits resembling tubercle to the naked eye." The youth was named Gibbons, and I saw him in consultation with Dr. Uhtoff, of Brighton, in January, 1884. This is probably the same affection which is now called *status lymphaticus*.

Alcoholic Paraplegia.

I believe I was the first to draw attention to the affection to which I give this appellation. I mentioned it in my course of lectures which I gave at Guy's, and which were in part published in the 'Medical Times and Gazette' in the year 1868. These lectures were subsequently published in a separate volume. Most of my examples were those of women, or, as I say, "ladies," as the patients were mostly publicans' wives and their husbands were landlords. I remember the expression, because Charcot, in quoting me, does not fail to use this very word. I considered the cord and membranes were involved, although, if the women entirely left off drinking spirits, these might recover themselves, but I believe a further investigation has shown that the disease is in the nerves and is a chronic neuritis.

Acromegaly.

This disease was fully described and the above name given to it by M. Marie in 1880, meaning en-

largement of the extremities. Cases, however, had evidently been seen before and described under other names; for example, Sir William Gull, under the name of "sporadic cretinism," had evidently come across varieties of it. Also Dr. Fagge, in the first edition of his 'Principles and Practice of Medicine,' edited by Dr. Pye Smith (since Fagge's decease), mentions under the same name, "cretinism," a very remarkable case which I had sent him and which is clearly a good example of the highly developed form.

He then describes the case with some of the history which I had given him. Instead of repeating this I will give it as it stands in my own notebook. In Fagge's work the reference is first edition, vol. i, p. 757. This was more than ten years before M. Marie had given it its present name. This is my description: "Miss Bousfield, aged 28 years, sent to me by Mr. Bader on February 9th, 1869, the oculist of Guy's Hospital, as she had gone to him for her loss of sight, but which he found not due to any local change in the eye, but to cerebral disease. He found complete amaurosis of the left eye and partial in the right; both eyes were prominent. She had suffered from blindness for nine months, and for some years had had neuralgia both in the face and head. She also had amenorrhœa for six years."

As to her general appearance, I was at first inclined to think it was a case of elephantiasis, but I soon was aware that such a case was perfectly novel to me, and felt sure that I had never seen a similar case before. Her mother came with her,

who was able to supplement anything which she herself had a dislike to disclose. My notes say that her appearance was most remarkable: the skin was thickened, distorting all her features, so as to give her a truly hideous expression, the nose being enlarged as well as the lips, and ears prominent somewhat like those of a negress. The hands were very large and the fingers like sausages; her mother also told me that the skin of the whole body was similarly disfigured. This change had been coming on for about six years. I saw her several times after this, and as all patients expect medicine, I gave her bromides and arsenic, which seemed to relieve her pains. At the end of a year the family went into the country (leaving Clapham), and I never saw her again, but I learned that she died about a year afterwards in a comatose condition; that was about six years after I first saw her.

I believe since this was written I have read that the pituitary body is always found affected, being much enlarged, and even the deeper tissues besides the skin, as the whole skeleton, the bones having undergone changes, especially the cranium.

On Marking or Furrows on the Nails as the Result of Illness.

I wrote a letter to the 'Lancet' on January 2nd, 1869, on this subject, and so little seemed to be known about it that my observations were regarded as original; in one sense they were, as I had seen them for myself and had observed the character of

the illness in which they occurred; at the same time there were descriptions of the various diseases of the nails, but none of them seem to tally with the one under consideration. I showed how it did *not* occur in any illness, but in those only in which during a short period the patient was at death's door and then recovered; or it might occur at the end of a temporary illness, if the patient reached the dying stage and then recovered.

One of the most marked cases of this kind, shown by me at the Pathological Society some years ago, about March 20th, 1888, was that of a gentleman who had had no ordinary illness, but had been a voyage to America, and both on the voyage there and on the return he had sea-sickness so badly that he became faint and pulseless, and was thought to be really dying. The white transverse marks on the nails were most apparent, which made it easy to reckon the time of their occurrence. Reckoning the growth of the whole nail as five or six months, the mark nearest the end of the finger showed the time of the outward voyage, as the one nearest the root of the nail showed the homeward voyage; the space between the marks I reckoned at six weeks which was the time the gentleman stayed at New York.

I had also a very marked case in a boy who recovered after a severe illness from perityphlitis. A photo with the lines well marked also was sent me by Dr. Norman Flower, of Yeovil, who had a patient with the same complaint and who was thought to be dying.

In the 'Lancet' for September 7th, 1878, I described a spotted condition of the hair on the beard giving the impression of the possibility of the presence of pediculi. Under the microscope it is seen to be a hair broken up into minute fibres and so resembles two brushes meeting together. It had been before noticed and received the name of *trichorrhexis nodosa*. A somewhat similar-looking affection is a parasitic disease called *piedra*.

In speaking of skin diseases, I believe I first drew attention to the streaks which sometimes occur on the limbs like the lineæ gravidarum on the abdomen of women, and these I called simply striæ or *lineæ atrophicæ*. I also gave the name of *verrucæ necrogenicæ* to the rough and warty states of the knuckles in those who are constantly making post-mortem examinations.

A letter of mine appeared in the 'Lancet' stating that asparagus was not a diuretic but an anuretic, that is, it suppressed the secretion, less urine being secreted after a meal of this vegetable. The fallacy arose from the odour of the urine due to some element of it combining with asparagin, the alkaloid of asparagus. This odour was supposed to show the increased action of the kidney, and therefore was a diuretic. This was a fallacious reasoning; as I endeavoured to show, the contrary was the case.

When I retired from practice in the autumn of 1901 I took up my abode at Hampstead. Wishing still to have an occupation I joined the Scientific

Society of Hampstead, and afterwards on the death of the President, Sir Richard Temple, I was elected in his place. It appeared that the society had only existed two or three years, and at that time a telescope had been given to it, but it was almost useless as the County Council had placed it on low ground near the ponds, and the narrow footpaths were quite dark at night.

As regards the ordinary meetings of the society, they took place at the Public Library, Stanfield House, quite close to my own residence. Weekly meetings were held here, and the members sometimes delivered lectures. Occasionally lectures were delivered by well-known scientific men, and these were usually given at the Town Hall as being a much larger room. One of the lectures which I myself gave was entitled, "The Relation of Science to Art," a second was on "Spirals," and a third on "Ambidexterity." Another little book I wrote in Hampstead, 'On the Relation of Man to Animals,' was founded on a lecture I had given long before (I think about 1891 [?]) at the request of the Church Congress, which met at Folkestone, the subject being "On the Relation of Christianity to Vivisection."

Since I have been here the Scientific Society has greatly developed; as we had a new telescope given us we wished to place it on the highest part of the Heath, near the "White Stone" pond; but the inhabitants near making great objections, we obtained an equally good place at the top of the water-tanks. We had also here the usual meteoro-

logical instruments, as a "barometer," thermometer, "rain-gauge," and glass globe to show the "daily sunshine." There is published every week an account in the local newspaper of all the markings of these instruments.

The same newspaper has kept an account also of some lectures I have given on subjects of local interest; one of these was a remarkable tree, which seemed to have been known for the past century, and called the "ear elm," as it had upon it a large "ear" about 2 ft. long; this had evidently been formed on the scar left from the falling off of a large branch. When the tree, which was much decayed, was cut down, I had the large "ear" cut into sections, and these I took to the Scientific Society, or rather I had the attendant to carry them as they were too heavy for me—in fact the great bulk of the "ear" required three men to lift it. As there was some doubt as to the nature of the curves of the "ear," the edges being brown like bark, I asked my friend, Mr. Blackman, professor at Cambridge, whenever in town to call and see me. He shortly did so, and soon determined that the brown ridges were composed of bark, not of wood.

Then I took the opportunity of examining the clothes worn by a man and little girl who were struck dead by lightning whilst standing under one of the shelters on the heath. I carefully examined the underclothing of the man, called, I think, "flannelette," and it was evident by the way the threads of this were pulled out that some great

force was acting beneath the sleeve, so that if I wished to imitate this it could only be done by placing some explosive within the sleeve; and in the same manner, if I wished to drag the child's boot off, it could only be done by putting into it some gunpowder or dynamite. And it was the same with other trees: in one struck in a second thunderstorm the bark was only torn off about twelve feet from the ground, and above that the white (real) wood was seen, but on the grass below were scattered hundreds of little branches of the overhanging bough. I saw that the lightning, failing to descend to the ground, had acted as an explosive force and broken the branch into these innumerable pieces. The lightning acted in the same manner under the name of a "fire-ball," the effect of which I afterwards witnessed; a workman in a garden saw a "ball of fire" run along in front of him and go out the other side; it there hit a wall and exploded, knocking out many of the bricks, so that the wall had to be repaired. This event occurred some years ago, but I found a similar account of it in an old Hampstead newspaper, and the marks of the new bricks were still visible.

Then having looked into the old superstition of "May" being "an unlucky month for marriage," and discovering that it had been also a Roman superstition, I found what I wanted in "Ovid's Fasti." This was printed in the 'Hampstead Express,' and as it afterwards appeared in a separate form in a journal which was only ephemeral, I print it here at length:

The Month of May Unlucky for Marriage.

This long-standing superstition is more evident than many others, owing to the different length of the marriage announcements which appear just before this eventful month, and immediately afterwards. At the end of April one and a half columns of the 'Times' is occupied with these advertisements. When May arrives this column is suddenly shortened, and the advertisements are often reduced to two in number, and these probably do not refer to marriages in May. When June arrives the number of marriages announced immediately increases, so that by the fourth of the month, the day on which I am now writing, they occupy a space in the paper more than a column in length.

Some years ago, being interested in the question of superstitions, I looked into this very ancient one, and the notes which I made I will now offer you for the interest of your readers. It might have been thought that it was the very month for marriages, when the year had thrown off its winter garment and all Nature was rejoicing in its new budding splendour, the trees and flowers gay in their brightest colours, and the birds mating and building their new homes. This is well expressed in the words of our old Elizabethan poet Spenser, in "The Shephearde's Calendar":

"Then came fair May, the fayrest mayd on ground,
Deckt all with dainties of her seasons pryde,
Lord! how all creatures laught when her they spide
And leapt and daunc't as they had ravisht beene!
And Cupid selfe about her fluttered all in greene."

Beautiful as all this seems, superstition prevents its fulfilment.

Superstitions, indeed, have the strongest hold on the human mind. No reason, no talking, can eradicate them. No religious creed can supersede them or destroy them. Missionaries tell us that amongst barbarous people, when converted to Christianity, the converts will not forego their superstitious practices. And so it is with the one to which I am alluding. It is as old as Christianity, and has as strong a hold on its followers as the one they have always professed. Look at the good people in Wales celebrating every year their Eisteddfod, when they repeat in their songs, dresses, and different customs the doings of their Celtic ancestors, the ancient Druids. Or look again at "Baal Fires" in Northumberland, where in many villages the people collect faggots on Midsummer Day, set them on fire, join hands, dance round it, jump over it, and, if they can, through it. My "Guide" to Northumberland mentions Whalton, a good sized village near Morpeth, where there are two ancient British camps, afterwards occupied by the Romans. In this village the people kindled a bonfire on Midsummer's Eve, danced around it and leapt through it, which the "Guide" says is evidently a relic of solar worship which had come down from Pagan times, the worship of Baal or the Sun-god.

And now for our present superstition. This is to be found in Ovid's "Fasti," the work which he wrote on the festivals of the Romans. He there states that in his time and earlier it was considered unlucky

to be married in May. Ovid was born a little before Christ, and therefore the tradition must be very old. He says certain days were unlucky for marriage as well as the whole month of May. These were called *atri* and marked black in the calendar; but widows might marry on those days. As regards May he says, "Mense malum Maio nubere vulgus ait," and then goes on to state that the most fortunate time to be married is in the month of June. It is worthy of notice that Ovid's words are—"The people say it is bad to marry in the month of May." This seems to imply that he had no belief in it himself. I think there can be no doubt that the upper class of Romans were less superstitious than we are. At least, I do not remember anything of this nature in what I have read in Cicero, Horace, or Julius Cæsar.

These particulars having created some little interest in the subject, I was asked to state more fully the reasons why the Romans considered May to be unlucky for marriages. Your readers therefore must be informed that Ovid wrote a work on the festivals or feasts of Rome called the "Fasti." He took each month separately and invoked the tutelary god to inform him about these feasts. He commenced with January, and called upon the god Janus, who opened the year. In this manner the poet went on until he reached the months of May and June, so called in honour of the two representative bodies in Rome, the elder and the younger assemblies called the "Majores" and the "Juniores." These months were intimately associated with the marriage rites, especially as Juno ruled over the

month bearing her name and, being the wife of Jupiter and the mother of children, held the guardianship of domestic life. Ovid says May was the month of the Lemuralia, or the Lemurian Festivals. These were instituted by Romulus in honour of his brother Remus, who died in this month, and were called the Remuria. Afterwards, the letters being changed, which was a common thing amongst the Romans, they were called the "Lemuria." The Lemures were the "manes" of the dead, which wandered about as ghosts over the world, terrifying the good people and haunting the wicked. The Romans had the superstition to celebrate festivals in their honour in the month of May. The solemnities continued three nights, during which time the temples of the gods were shut and marriages prohibited. The people performed many curious ceremonies to drive the ghosts away.

This is what Ovid says:

"Mox etiam Lemures animas dixere silentum;
Hic sensus verbi, vis ea vocis erat.
Fana tamen veteres illis clausere diebus,
Ut nunc ferali tempore operta vides.
Nec viduæ tædis eadem, nec virginis apta
Tempora; quæ nupsit, non diuturna fuit.
Hac quoque de causa, si te proverbia tangunt,
Mense malum Maio nubere vulgus ait."

I shall not attempt to translate this literally; but, as far as I understand the verses, they might be construed into the following meaning:

"Moreover, also the Lemures (the manes) spoke of the souls of the silent (the dead).
The one is the memory of the word; the other was the power of the voice.

At these times the old temples are closed.
So now, at this time of solemnity, you see them shut up.
Neither at these times do widows marry or are virgins united.
If she did marry the marriage would not be lasting.
It is for this reason, if proverbs touch you,
That people say it is bad to marry in May."

There is one edition of the fasti in which the text gives "malas" instead of "malum," which being feminine and plural, would probably mean "They are bad women who marry in May." This would in no wise affect the dictum "that it is unlucky to marry in May."

I might add that it was the custom, when the bride left the paternal roof and control, to salute her with a shower of rice, a practice still continued.

Many things of this kind, like bowing to the new moon, a still older worship, may be regarded as foolish and amusing; but it is different with the marriage superstition, which has a practical side, well known to many men who have had to suffer in consequence much inconvenience and even hardships.

I may add to this account by saying that having spoken to my friend, Dr. Payne, the learned librarian of the Royal College of Physicians, on the subject, he informed me that Plutarch, the historian, had discussed the matter. He lived nearly a hundred years after Ovid, and discussed the latter's reasons for his statements. Plutarch wrote a book styled the 'Moralia,' in which is a chapter called "Quæstiones Romanæ," or "Roman Questions." This was translated by Dr. Philemon Holland in 1603, and this I quote. It is interesting to note the quaint style of his day.

He begins as follows: "Why did not the Romans marry in the month of May? Is it for that it cometh between April and June? Whereof the one is consecrated unto Venus and the other to Juno, who are both of them goddesses which have the care and charge of wedding and marriages, and therefore think it good either to go somewhat before, or else to stay a while after. Or it may be that in this month they celebrate the greatest expiatory sacrifice of all others in the year? For even to this day they fling off from the bridge into the river the images and portraitures of men, whereas in old time they threw down men themselves alive. And this is the reason for the custom nowadays that the priestess of Juno name Flaminia should be always sad and heavy, as it were a mourner, and never wash nor dress nor trim herself. Or what and if we say, it is because many of the Latin nations offered oblations unto the dead in this month; and peradventure they do so, because in this very month they worship Mercury; and in truth it beareth the name of Maia, Mercury's mother. But may it not be rather, for that as some do say, this month taketh that name from Majores, that is to say, ancients; like as June is termed so from Juniores, that is to say, younkers. Now this is certain, that youth is much meeter for to contract marriage than old age; like as Euripedes said very well:

> "As for old Age, it Venus bids farewell,
> And with old folk Venus is not pleased well."

The Romans therefore married not in May, but stayed for June, which followeth immediately after May."

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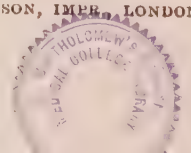

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